This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

7

1837
ARTES
VERITAS
SCIENTIA
LIBRARY OF THE
UNIVERSITY OF MICHIGAN
E PLURIBUS UNUM
TUEBOR
SI QUAERIS PENINSULAM AMOENAM
CIRCUMSPICE

EDINBURGH
MEDICAL AND SURGICAL
JOURNAL:

EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND
PHARMACY.

BRISTOL GENERAL HOSPITAL

VOLUME FIFTY-FIFTH.
1841.

16745

EDINBURGH:
PRINTED FOR ADAM AND CHARLES BLACK:
LONGMAN, ORME, BROWN, GREEN, & LONGMANS, LONDON:
JOHN CUMMING, AND HODGES & SMITH, DUBLIN.

1841.

B. a.

PRINTED BY JOHN STARK, EDINBURGH.

THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

No. CXLVI.

CONTENTS.

PART I.—Original Communications.

Art. I. On the Anatomical Relations of the Blood-vessels of the Mother to those of the Fœtus in the Human Species. By John Reid, M. D., ... Page 1

II. On some points in the Anatomy the of Medulla Oblongata. By John Reid, M. D., ... 12

III. The properties and influence of Arteries on the Circulation of Blood. By G. Calvert Holland, M. D. . . 17

IV. An Inquiry into the Mechanical Functions of the Ear. By James Syme, M. D. 48

V. Case of Aortal Aneurism which opened into the Vena Cava Superior, opposite the entrance of the Vena Azygos. By William Young, M. D. 63

VI. Two Cases of Impracticable Labour arising from Malacosteon of the Pelvis, in which the Cæsarean Operation was performed; accompanied with Practical Observations. By Thomas Radford, M.D. 67

VII. A Description of a Double Monocephalic Human Monster, which was transmitted to this country from South America, by Robert Mackay, Esq. British Consul at Maracaylia, Venezuela. By Eric Mackay, M. D. 76

VIII. Malta, considered with reference to its eligibility as a place of Residence for Invalids. By Francis Sankey, M.D. . . 81

IX. On the Diurnal Variations of the Pulse in Disease. By William Augustus Guy, M. B. Cantab. . . . 90

X. Observations on the Use of Piperine in the Treatment of Intermittent Fever. By Robert Hartle, M. D. . . 98

XI. Observations on the Therapeutic Action of Croton Oil in certain Nervous Disorders. By P. S. K. Newbigging, M.D., 100

XII. Case of Amputation of the Neck of the Womb followed by Pregnancy; with Remarks on the Pathology and Radical Treatment of the Cauliflower Excrescence from the Os Uteri. By James Y. Simpson, M. D. . . . 104

XIII. On Sanguineous Tumours on the Scalp in New-born Children. By Francis Black, M. D. . . . 112

XIV. Two Cases of Rupture of the Carotid Artery from Sphacelus. By Mr C. J. Mill, Surgeon, . . . 125

XV. Cases Illustrative of the Division of Tendons. By William Rhind, Surgeon, ...

XVI. Appendix to Dr J. Reid's Paper on the Anatomical Relations of the Blood-vessels of the Mother to those of the Fœtus, . 135

PART II.—Critical Analysis.

Art. I. The Russian Military Phramacopœia. By Sir James Wylie, Bart. 137

II. Odontography, or a Treatise on the Comparative Anatomy of the Teeth; their physiological relations, mode of developement, and microscopic structure, in the Vertebrate Animals, illustrated by upwards of 150 Plates. By Richard Owen, F.R.S. . 14

170537

ART. III. First and Second Annual Reports of the Registrar-General of Births, Deaths, and Marriages in England, with Appendices, . 156
IV. Transactions of the Medical and Physical Society of Bombay. Vol. I. 194
V. Outlines of a Course of Lectures on Medical Jurisprudence. By THOMAS STEWART TRAILL, M.D. . . 216
VI. The Elements of Materia Medica; comprehending the Natural History, Preparation, Properties, Composition, Effects, and Uses of Medicines. By JONATHAN PEREIRA, F.R.S. . 219
VII. 1. Report on the result of the Operations for the Cure of Squinting, performed at the Royal Westminster Ophthalmic Hospital, between the 18th April and 30th October 1840. By CHARLES W. G. GUTHRIE, Junior.
2. A Practical Treatise on the Cure of Strabismus, or Squint, by Operation and by milder Treatment; with some new Views of the Anatomy and Physiology of the Muscles of the Human Eye. By P. BENNET LUCAS, M.R.C.S.L. . . 227
VIII. 1. Manual of General Anatomy. By J. F. MECKEL.
2. The Anatomist's Manual; or a Treatise on the manner of preparing all the Parts of Anatomy; followed by a complete Description of these Parts. By J. P. MAYGRIER, M.D.P.
3. The Anatomist's Vade Mecum: a System of Human Anatomy. By ERASMUS WILSON.
4. Demonstrations of Anatomy. By GEORGE VINER ELLIS, 244
IX. On the Influence of Artificial Light in causing impaired vision, and on some methods of preventing or lessening its injurious action on the Eye. By JAMES HUNTER, M.D. . 247

PART III.—MEDICAL INTELLIGENCE.

On the Functions of the Ganglionic Nerves, . . 249
Acidity of the Menstrual Secretion the cause of its non-coagulability, 250
Case of Irregular Disposition of the Aorta and Pulmonary Artery, ib.
Case of Absence of the Pulmonary Artery, . . 251
Case of Congenital Hernia of the Heart and Stomach, . . ib.
On the Developement of Cancer in the Veins, and the Transmission of Cancer from Man to the Lower Animals, . . . ib.
Sudden Death from Rupture of the Spermatic Vein, . 253
On the occurrence of Variolous Pustules on the Internal parts of the Body, 255
On the Exanthemata and Enanthemata, . . ib.
On Rabies, ... 256
On the Employment of Blood-letting and Opium in Acute Phlegmasia, 257
Employment of Solution of Common Salt as an Eye Lotion in Ophthalmia, 260
On the Antisudorific Properties of Tannin, . . ib.
Case of Ascites cured by Bandaging, . . ib.
Of Abdominal Tympanitis, . , , 261
Case of Recovery from a severe injury of the Head, . ib.
Case of Enormous Empyema successfully treated by Operation, . 262
On the Efficacy of Cod-liver Oil in Caries and Scrofula, . ib.
On the Cure of Fissures of the Anus by Rhatany Root, . 263
Successful Case of Cure of General Articular Deformity, . 264
Case of Amputation at the Shoulder-joint with removal of the Scapula, 265
Poisoning by the Vapours of Antimony, . . ib.
Lemon Juice the antidote for poisoning by Hyoscyamus, . ib.
Substance employed in 194 cases of Poisoning in Paris, from 1832 to 1838, 266
On the Cure of Procidentia Uteri by Suture of the External Labia, ib.
On the danger of injecting fluids into the Uterus, . . 267
Delivery of Four Children at a Birth, . . 268
Case in which Milk was expectorated after each Delivery, . ib.
Official Document as to the Re-vaccinations over France, . ib.
On Infanticide in France, . . . 269
Statistics of the Bloomingdale Asylum for the Insane, . . ib.
Report by the Royal College of Surgeons of Edinburgh, . 270
The late Mr Musgrave, Student of Medicine, . . 284

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr Boyd, Dr Vose, Mr Benjamin Bell, and Mr S. S. Stanley.

The following works have been received :—

Organic Chemistry; on its Applications to Agriculture and Physiology. By Justus Liebig, M. D., Ph. D., F. R. S., M. R. I. A., Professor of Chemistry in the University of Giessen, &c. Edited from the Manuscript of the Author by Lyon Playfair, Ph. D. London, 1840. 8vo. Pp. 387.

Lectures on the Morbid Anatomy of the Serous and Mucous Membranes. In two volumes. By Thomas Hodgkin, M. D., Member of the Lyncean Academy of Rome, &c. &c. Vol. II. Part I. of the Mucous Membranes. London, 1840. 8vo. Pp. 541.

A Practical Treatise on the Functions and Diseases of the Unimpregnated Womb. Illustrated by Plates, &c.; with a Chapter on Leucorrhœa. Fluor Albus, or Weakness. By Charles Waller, M. D., &c. London, 1840. 8vo. Pp. 200.

Remarks on the Surgical Practice of Paris. Illustrated by Cases. Being a Thesis to which a gold medal was assigned by the Senatus Academicus of the Edinburgh University at the Graduation of 1840. By W. O. Markham, M. D. London, 1840. 8vo. Pp. 114.

Practical Remarks on the Discrimination and Appearances of Surgical Disease; with an Appendix containing the Descriptive Catalogue of the Author's Collection in Pathological Anatomy; and the Hunterian Oration for 1837. By John Howship, Surgeon to Charing Cross Hospital, &c. &c. London, 1840. 8vo. Pp. 420.

Guy's Hospital Reports, No. IX. October 1840. Edited by George H. Barlow, M. A. and L. M. Trinity College, Cambridge; and James P. Babington, M. A. Trinity College, Cantab. &c. London, 1840.

Human Physiology. By John Elliotson, M. D. Cantab. F. R. S.; with which is incorporated much of the elementary part of the Institutiones Physiologicæ of J. F. Blumenbach, M. D., F. R. S., illustrated with numerous wood-cuts. Fifth Edition. London, 1840. 8vo. Pp. 1194.

The Pathology and Diagnosis of Diseases of the Chest; with an Appendix. By Charles J. B. Williams, M. D., F. R. S. 4th Edition. London, 1840. 8vo. Pp. 331.

A Discourse on the Phenomena of Sensation as connected with the Mental, Physical, and Instinctive Faculties of Man. By James Johnstone, M. D., F. R. C. P. &c. &c. London, 1840. 8vo. Pp. 264.

The Anatomy of the Arteries of the Human Body; with its applications to Pathology and Operative Surgery. In Lithographic Drawings. By Richard Quain, Professor of Anatomy in University College, &c. &c. Part I. and II. 1840. 8vo. Pp. 90.

An Essay on the Chemical, Botanical, Physical, and Parturient Properties of the *Secale Cornutum*, with an Engraving. By T. A. Wardleworth, Surgeon. London, 1840. Duodecimo. Pp. 69.

Spinal Diseases; with an Improved Plan of Cure. Including what are commonly called Nervous Complaints, and Numerous Examples from upwards of 150 Cases. By John Hey Robertson, M. D., Surgeon, &c. &c. Glasgow, 1840. 8vo. Pp. 16.

The History of the early and present State of the Venereal Disease examined. By G. Hume Weatherhead, M. D., &c. &c. London, 1841. 12mo. Pp. 256.

Illustrations of the Comparative Anatomy of the Nervous System. By Joseph Swan. Part VI. London, 1840. 4to. Pp. 262.

An Introductory Lecture on Surgery, delivered on Wednesday, 4th November 1840. By James Miller, F. R. C. S. E., &c. &c. Edinburgh, 1840. 8vo. Pp. 23.

Practical Observations on the Pathology and Treatment of Stricture of the Urethra; with Cases. By Robert Wade, Senior Surgeon to the Westminster General Dispensary, &c. &c. London, 1841. 8vo. Pp. 149.

Elements of Chemistry; including the actual state and Prevalent Doctrines of the Science. By the late Edward Turner, M. D., F. R. S. L. & E. Seventh Edition. Edited by Justus Liebig, M. D., and William Gregory, M. D., F. R. S. E., Professor of Chemistry, King's College, Aberdeen. London, 1840. 8vo. Pp. 988.

On Scientific Medicine, and its relations to, and claims upon, society at large; being an Address read before the President and other Members of the North of England Medical Association. By William Elliot, M. D. &c. With an Appendix. Carlisle, 1840. 8vo. Pp. 44.

An Inquiry into the efficacy of Digitalis in the Treatment of Idiopathic Epilepsy. By Edmond Sharkey, A. B., M. D. &c. London, 1841. 8vo. Pp. 80.

A Treatise on Stricture of the Urethra; containing an Account of Improved Methods of Treatment; with an Appendix on Dilatation by fluid pressure in the Treatment of Urinary Calculus and other Diseases. By James Arnott, M. D. &c. 2d Edition. London, 1840. 8vo. Pp. 234.

A Treatise on the Nervous Diseases of Women; comprising an Inquiry into the Nature, Causes, and Treatment of Spinal and Hysterical Disorders. By Thomas Laycock, M. D. London, 1840. 8vo. Pp. 356.

A Treatise on the Sympathetic Relation between the Stomach and the Brain, and between the Digestive and the Nervous Systems, in the causation and cure of Diseases. By Charles Wightman, M. D., &c. &c. London, 1840. 12mo, pp. 192.

Fifth Report of Inspectors of Prisons. III. Southern and Western Districts. London, 1840.

Memoranda regarding the Royal Lunatic Asylum, Infirmary, and Dispensary of Montrose. Prepared, at the request of the Managers. By Richard Poole, M. D., F. R. C. P. E., Medical Superintendent of the Montrose Lunatic Asylum. Montrose, 1841. 8vo, pp. 217. Appendix. Pp. 60.

Practical Remarks on the New Operation for the cure of Strabismus or Squinting. Illustrated with Lithographic Engravings. By Edward W. Duffin, Graduate of the University of Edinburgh, &c. London, 1840. 8vo, pp. 147.

Medicinisches Schriftsteller Lexicon der jetzt Lebenden Verfasser. Von. Dr Adolph Carl Peter Callisen, Ritter des Dannebrogordens, u. s. w. Nachtrag Enthaltend: Berichtigungen, Erganzungen, die Neuere Literatur, und die seit, 1830, verstorbenen Medicinischen Schriftsteller. Mit einigen Beitragen, Von den Herren, Doctoren: dem Hofrathe Choulant, dem Leibchirurgen Koberwein, dem Archiater und Etatsrathe, A Von Schonberg. Achtundzwanzigster Band F—Hir. Copenhagen, 1840.

Vital Statistics of Scarborough. By John Dunn, Surgeon. London, 1840. 4to. Pp. 11.

Essays and Heads of Lectures on Anatomy, Physiology, Pathology, and Surgery. By the late Alexander Monro, Secundus, M. D.; with a Memoir of his Life by his Son and successor. Illustrated by Engravings. Edinburgh, 1840. 8vo. Pp. 132.

The London Medical Gazette for the Session 1839—1840. No. 52, 18th September 1840, for the Session 1840—1841. No. 1, 25th September 1840, to No. 12, 11th December 1840, (680.)

Gazette Medicale de Paris, No. 38. Samedi, 19th Septembre 1840, a No. 49. Samedi, 5th Septembre 1840. Tome viii.

The Medico-Chirurgical Review and Journal of Practical Medicine, No. 66. Oct. 1840. 26th Decennial Series. Edited by James Johnson, M. D., &c. and Henry James Johnson, Esq. London, 1840.

The British and Foreign Medical Review, or Quarterly Journal of Practical Medicine and Surgery. Edited by John Forbes, M. D., &c. London. No. 20. October 1840.

Weekly Tables of Mortality for the Metropolis, No. 35, 5th September 1840, to No. 48, 5th December 1840.

THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

No. CXLVII.

CONTENTS.

PART I.—ORIGINAL COMMUNICATIONS.

ART. I. Jaundice from Non-Elimination, together with Remarks on the Pathological Condition and Chemical Nature of the Bile. By W. H. LOWE, M. D. . . . Page 285

II. Pathological Contributions. By Dr BOYD, . . 304

III. On some Vicarious Actions of the Human Body, in Health and in Disease. By J. R. W. VOSE, M. D. . . 322

IV. Report on the effects of the removal of the 65th, 36th, 69th, 37th, and 8th Regiments, from the West Indies to Nova Scotia, in the years 1838 and 1839. By J. ELLIOTT, M. R. C. S. . 335

V. Brief Notes on the Lake-Fever of Canada. By THOMAS STRATTON, M. D. 348

VI. Contributions to Pathology.—Rupture of the Liver—Rupture of the Spleen—Empyema,—Epilepsy and Apoplexy. By ALEXANDER KILGOUR, M. D. 352

VII. On the Cure of Strabismus. By THOMAS ELLIOT, M. R. C. S. L. 370

VIII. Observations on Corpora Lutea. By ROBERT PATERSON, M. D. 395

IX. Account of a Male Monster with Two Heads. By SAMUEL BROMILOW, Esq., M. R. C. S. L. . . . 435

X. On the Order of succession in which the Vital Actions are arrested in Asphyxia. By JOHN REID, M. D. . . 437

XI. Collection of Fluid in the substance of the Right Lobe of the Thyroid Body. By J. MASSEY, Esq. . . . 453

XII. Cases of Cerebral Disease. By BENJAMIN BELL, Esq., F. R. C. S. 454

XIII. Report of the Labour of Mrs C. in whom the Neck of the Uterus was amputated by Professor Simpson. By ROBERT LEWINS, Junior, M. D. . . . 469

PART II.—CRITICAL ANALYSIS.

ART. I. Researches in Embryology. By MARTIN BARRY, M. D. 474

II. Traité Pratique d'Auscultation, ou exposé méthodique des diverses applications de ce mode d'examen à l'etat physiologique et morbide de l'economie. Par M. BARTH, et M. HENRI RAYER, M. D. 486

III. Vital Statistics of Scarborough. By JOHN DUNN, Surgeon, . 487

IV. 1. Observations on the Oriental Plague, and on Quarantine, as a means of arresting its Progress. By JOHN BOWRING.

2. Quarantine Laws, and their Abuses and Inconsistencies. By ARTHUR HOLROYD, Esq. . . . 491

ART. V. 1. Practical Observations on Diseases of Women, Illustrated with Cases and Explanatory Plates. By WILLIAM JONES, M. R. C. S.
2. A Practical Treatise on the Function and Diseases of the Unimpregnated Womb, Illustrated by Plates, &c. with a Chapter on Leucorrhœa, Fluor Albus, or Weakness. By CHARLES WALLER, M. D. . . . . . Page 519
VI. A Treatise on the Sympathetic Relation between the Stomach and the Brain, and, throughout, between the Digestive and the Nervous Systems in the Causation and Cure of Diseases; with an Appendix, containing a few Observations on certain points connected with the Treatment of Chronic Disease and its attendant Debility. By CHARLES WIGHTMAN, M. D. . . . 526
VII. The Retrospect of Practical Medicine and Surgery for the year 1840. Being an Analysis of the British and Foreign Medical Journals and Transactions; or a Selection of the Latest Discoveries and most Practical Observations on the Practice of Medicine, Surgery, and the Collateral Sciences for the past year, made chiefly with reference to the Treatment of Disease. By W. BRAITHWAITE, M. R. C. S. 542

PART III.—MEDICAL INTELLIGENCE.

Experiments on Animals with the view of ascertaining the effect of Ligature of the Carotid Arteries, ... 543
On the varying proportion of some of the principles of the Blood in Disease, 544
On the existence of Fibrine in the Blood in the shape of Globules, . 545
On the Contractility of the Blood-vessels, ... ib.
On the Functions of the Roots of the Spinal Nerves, . . 546
That the Physical and Moral qualities observed in Adult Age are evinced at Birth, ... ib.
On the Origin and Mode of Development of Spermatic Animalculæ, . 547
Abnormal situation of the Right Kidney, . . 549
Case of Obliteration of the Vena Cava Inferior, . . ib.
Case of Cyanosis in an adult from the Foramen Ovale being open, . 550
Remarks on the Tinctura Opii Ammoniata of the Edinburgh Pharmacopœia, ib.
Case of Complete Paralysis of the Fifth Pair of Nerves, . . 551
Successful Treatment of Croup by repeated Emetics, . . 553
Removal of one of the Bones of the Metacarpus, the corresponding finger being preserved, ... ib.
A Hair extracted from the anterior chamber of the Eye, . ib.
Case of Section of the right anterior Cord of the Spinal Marrow between the sixth and seventh Cervical Vertebræ, ... ib.
On the Influence of Supra-orbital Wounds in the production of Amaurosis, 554
Case of Dislocation of the Cervical Vertebræ cured, . . ib.
Violent Inflammation of the Urethra apparently caused by eating Frogs, 555
On the length and strength of the Umbilical Cord at the full term of Pregnancy, 556
On the best and surest mode of inducing Premature Labour, . ib.
Successful case of Cæsarean Section, ... 557
Case of Poisoning by Tobacco, ... 558
Case of Poisoning with the Acetate of Lead, in which the poison was detected in the Urine, ... ib.
On the best mode of Treating Cases of Poisoning by Arsenic, . 559
Resolutions of College of Physicians of Edinburgh on Messrs Hawes' and Warburton's Bills for Medical Reform, . . . 560
Gloucestershire Medical Association, ... 563
Petition of the Gloucestershire Medical Association, . . 565
Military Surgery, ... ib.
Harveian Society.—Notice to Students of Medicine, . . 567

NOTICES TO CORRESPONDENTS.

Communications have been received from Dr **Holland**, Dr **Marr**, Dr **Stark**, and Mr **Mackay**.

The following Publications have been received:—

The Domestic Management, necessary in aid of Medical Treatment, for the Cure of Diseases. By Anthony Todd Thomson, M. D., F. L. S., &c. &c. London, 1841. 8vo. Pp. 506.

The Library of Medicine. Arranged and edited by Alexander Tweedie, M. D., F. R. S., &c. Vol. vi. Midwifery, by Edward Rigby, M. D., &c. Post 8vo. Pp. 314.

The Cause and Treatment of Curvature of the Spine, and Diseases of the Vertebral Column. Illustrated with Cases and Plates. By E. W. Tuson, F. R. S., &c. London, 1841. 8vo. Pp. 283.

On the Diseases of the Hip-Joint; with Observations on Affections of the Joints in the Puerperal State, with Plates. By William Coulson, Surgeon to the Magdalen Hospital, &c. &c. Second Edition, with alterations and additions. London, 1841. 8vo, Pp. 211.

Practical Essays. By Sir Charles Bell, K. H., M. D. Gott. F. R. S. L. et E., Professor of Surgery in the University of Edinburgh. Edinburgh, 1841. 8vo. Pp. 104.

Report to the Secretary of State for the Home Department. From the Poor Law Commissioners on the Training of Pauper Children; with Appendices. London, 1841. 8vo. Pp. 421.

A Practical Treatise on the Diseases of the Liver and Biliary Passages. By William Thomson, M. D., Fellow of the Royal Colleges of Physicians and Surgeons, and one of the Physicians to the Royal Infirmary. Edinburgh, 1841. 8vo. Pp. 306.

A Few Hints addressed to Medical Students about to visit the Parisian Hospitals. By a Physician. London, 1841. 12mo. Pp. 56.

The Mineral Springs of England, and their Curative Efficacy: with Remarks on Bathing, and on Artificial Mineral Waters. By Edwin Lee, Esq., M. R. C. S. London, 1841. 12mo. Pp. 124.

The Surgical Anatomy of Inguinal Herniæ, the Testis and its Coverings. By Thomas Morton, one of the Demonstrators of Anatomy in University College London, &c. London, 1841. Large 8vo. Pp. 330.

A Practical Treatise on the Venereal Disease, founded on six Lectures on that subject, delivered in the session of 1838–39, at the Aldersgate School of Medicine, with Plates. By F. C. Skey, F. A. S., Assistant-Surgeon at St Bartholomew's Hospital, &c. London, 1840. 12mo. Pp. 195.

Elements of Obstetric Medicine; with the Description and Treatment of some of the Principal Diseases of Children. By David D. Davis, M.D., &c. Second Edition, illustrated by Engravings on Wood. London, 1841. 8vo. Pp. 1022.

A Series of Anatomical Sketches and Diagrams, with Discriptions and References. By Thomas Wormald, Assistant-Surgeon and Demonstrator of Anatomy in St Bartholomew's Hospital, and Andrew Melville Macwhinnie, Teacher of Practical Anatomy at St Bartholomew's Hospital. Part 4. London, 1841. 4to. Pp. 33–39. Plates 15 to 18.

The Fifty-Fifth Report of the Visiting Justices of the County Lunatic Asylum at Hanwell. London, 1840. Post 8vo. Pp. 93. Medical Report by J. Conolly, M.D.

The Touchstone of Medical Reform; in three Letters addressed to Sir Robert Harry Inglis, Bart. M.P. By Joseph Henry Green, F.R.S., Professor of Anatomy to the Royal Academy, &c. Lond. 1841. 8vo. Pp. 72.

Annales de la Chirurgie Francaise et Etrangère. Par MM. Begin, Marchal, Velpeau, et Vidal. Numero 1. Janvier 1841. 8vo. Pp. 128.

The London Medical Gazette for the Session 1840–1841. No. 13. 18th December 1840, (681,) to No. 25, (693,) 13th March 1841.

Gazette Medicale de Paris, No. 50 et 52, et No. 1. Samedi, 2 Janvier 1841, à No. 2, Samedi, 13 Mars 1841.

The Medico-Chirurgical Review, and Journal of Practical Medicine. No. 67. January 1841, 27th of a Decennial Series. Edited by James Johnson, M. D., &c. and H. J. Johnson, Esq. London, 1841.

Nuces Philosophicae, or the Philosophy of Things as developed from the Study of the Philosophy of Words. By Edward Johnson, Esq., Author of Life, Health, and Disease. London, 1841. No. 1, January; No. 2, February 1841. Pp. 88.

Edinburgh Journal of Medical Science. Edited by John Rose Cormack, M. D. Edin. No. 1, January; No. 2, February 1841.

THE
EDINBURGH
MEDICAL AND SURGICAL JOURNAL.
1. JANUARY 1841.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*On the Anatomical Relations of the Blood-vessels of the Mother to those of the Fœtus in the Human Species.* By John Reid, M. D., F. R. C. P. E., Lecturer on the Institutes of Medicine, President of the Anatomical Society, Superintendant of Pathological Anatomy in the Royal Infirmary, &c.

The particular manner in which the blood-vessels of the mother and fœtus are arranged on the inner surface of the uterus and in the interior of the placenta, by which their contained fluids act and react upon each other and the blood of the fœtus is rendered fit for the process of nutrition, has been the subject of much discussion, and of much investigation. That the blood-vessels of the uterus and placenta communicate with each other, and that a mutual interchange of blood can readily take place between them, appears to have been the prevailing supposition among the earliest cultivators of physiological science. This opinion was afterwards strengthened by the results which Cowper,[*] Vieussens,[†] Noortwyk,[‡] and others, supposed they had obtained from injections of impregnated uteri; and also by the details of cases in which it was believed that the blood-vessels of the fœtus had been drained by profuse or fatal hæmorrhage from the mother.[*] Haller has adduced several arguments to show that there must be some direct vascular connection between the mother and fœtus;[†] and Senac maintained that the uterine vessels of the mother, and the placental arteries of the fœtus, poured their blood into a cellular tissue in the placenta, and that the fœtal placental veins took their origin from the interior of these cells.[‡] Flourens is the only author of any reputation in modern times who has alleged that there is any direct communication between the blood-vessels of the mother and the fœtus; but there cannot be a doubt that this opinion is erroneous, and ought now to be totally abandoned.[§]

---

[*] The Anatomy of the Human Body. The fifty-fourth Table. 1698.

[†] Dissertatio de Structura et Usu Uteri Placentæ Muliebris, in Genevensi Verheyenii editione. Vieussens' observations were made on the bitch.

[‡] Uteri Humani Gravidi Anatome et Historia, p. 10. 1743.

Satisfactory evidence was adduced by Monro *primus*,[||] the two Hunters,[¶] Monro *secundus*,[**] and his brother, Dr D. Monro,[††] and by Wrisberg,[‡‡] that there is no vascular continuity between the blood-vessels of the mother and the placental vessels of the fœtus, and this has since been most fully confirmed by the testimony of numerous accurate and careful observers.

A most important advance was made in our knowledge of the anatomical relation of the blood-vessels of the mother and fœtus by the labours of the Hunters. They satisfied themselves that the umbilical arteries terminate in the umbilical veins, and not in the vessels of the uterus, and that the blood in the umbilical arteries "passes from the arteries into the veins as in other parts of the body, and so back again into the child." They further observed, that numerous small curling arteries, the largest being about the size of a crow-quill, passed from the inner surface of the uterus, that they penetrated the decidua, and opened into the interstices between the fœtal blood-vessels of the placenta.[*] Prolongations from the uterine sinuses were also traced through the decidua, and were observed to terminate in the placenta in the same manner as the curling arteries, so that " in the umbilical portion of the placenta the arteries terminate in veins by a continuity of canal, whereas in the uterine portion there are intermediate cells, in which the arteries terminate, and from which the veins begin." It was therefore concluded, that the blood of the mother was poured by the curling arteries into a kind of cellular tissue, filling up the intervals between the ramifications of the fœtal placental vessels, from which it returns to the uterine sinuses of the mother through their placental prolongations, after having acted upon the blood of the fœtus through the thin walls of the umbilical placental vessels.

---

[*] Vide a case by Mery, Memoires de l'Academie Royale des Sciences. 1708.

[†] Elementa Physiologiæ, Tom. viii. p. 255–58. 1778. We are informed by Dr D. Monro (Edinburgh Physical and Literary Essays, Vol. i. p. 451, 1761,) that Haller, in a private conversation, stated to him, that subsequently to the expression of this opinion, he had examined three impregnated uteri, and found nothing like a direct communication between the uterine and fœtal placental vessels, "so that he now believed that there was no such anastomosis as was alleged."

[‡] Traité de la Structure du Cœur, Tom. ii. p. 68. 1749.

[§] Cours sur la Generation, p. 130. Paris, 1836. Flourens states that this communication exists in those animals only which have a *single* placenta, as in the human species, the carnivora, and the rodentia.

[||] Edinburgh Medical Essays, Vol. ii. p. 102. Third edition. 1747.

[¶] John Hunter "On the Animal Economy," 1794; and William Hunter, "The Anatomical Description of the Human Gravid Uterus and its contents." 1794.

[**] Edinburgh Physical Essays, Vol. i. p. 481.

[††] Idem liber, p. 456.

[‡‡] Commentationum Medici, &c. p. 46 and 312. 1800. See more especially his notes to the Primæ Lineæ Physiologiæ of Haller, Caput xxxi.

The existence of these utero-placental vessels has, however, within the last few years, been repeatedly called in question, both in this country and on the continent, and it has been asserted that the blood of the mother cannot be carried into the interior of the placenta, that the placenta is entirely a fœtal organ, and that the blood-vessels of the uterus and placenta cannot intermingle. The supporters of this view of the nature of the connection of the uterus and placenta have been Lauth,[†] Dr R. Lee,[‡] Velpeau,[§] Dr Radford,[‖] Seiler,[¶] Ramsbotham,[**] Mr Millard,[††] and Mr Noble.[‡‡] From the extent and respectability of this adverse testimony, serious doubts of the correctness of the description given by the Hunters began to be entertained by many who had hitherto placed implicit confidence in it. Evident signs of a rapid return to the Hunterian views have, however, now manifested themselves, and this has been effected not only by the confidence due to the known accuracy of the Hunters, but also by the late observations of Weber,[§§]

---

[*] These curling arteries have been figured by Albinus (Uteri Gravidi, Tab. vii); by Dr D. Monro and Monro *secundus* (Opera cit.); by William Hunter (The Anatomy of the Gravid Uterus Exhibited in Figures, plates 28, 29, and 30. 1774); and by John Hunter (opus cit).

[†] Répertoire Général D'Anatomie et de Physiologie, Tom. i. p. 75. 1826. Lauth believed that the union of the uterus and placenta is effected by means of vessels, but these are lymphatics and not blood-vessels.

[‡] Philosophical Transactions for 1832, p. 57.

[§] Embryologie ou Ovologie Humaine, p. 63. Paris, 1833, " J'ai cherché mais en vain ces vaisseux utero-placentaires," p. 69.

[‖] On the Structure of the Human Placenta. Manchester, 1832.

[¶] Die Gebärmutter und das Ei des Menchen, S. 31. Dresden, 1832.

[**] Medical Gazette, Vol. xiii. p. 613, 1834.

[††] Ibid. Vol xiv. p. 654.

[‡‡] Ibid. p. 810.

[§§] Hildebrandt's Handbuch der Anatomie des Menchen. Vierter Band. S. 495, 1832.

Dr Hugh Ley,[*] Eschricht,[†] Mr Owen,[‡] and others; and by the report made by Mr Mayo and Mr Stanley on the preparations of impregnated uteri contained in John Hunter's Museum.[§]

Though the researches of Weber and Eschricht have led them to adopt the description of the Hunters, as far as relates to the existence of the utero-placental vessels, yet their account, as we shall afterwards see, of the structure of the interior of the placenta, differs considerably, not only from the Hunters, but also from each other.

Having made these preliminary remarks, we shall now proceed to give the details of an examination of an impregnated uterus which we had lately an opportunity of making, as they appear to us to throw additional light on these intricate questions. The uterus was obtained from the body of a woman who died suddenly about the middle of March 1839, from apoplexy occasioned by inflammatory softening of the brain, while near the seventh month of pregnancy. The uterus was first opened, the fœtus was then removed, and a portion of the uterus with the whole of the adhering placenta was carefully secured. The principal facts, contained in the description which I am about to give, were not made out until August 1840. On separating the adhering surfaces of the uterus slowly and cautiously under water, I satisfied myself, but not without considerable difficulty, of the existence of the utero-placental vessels described by the Hunters. After a portion of the placenta had been detached in this manner, my attention was attracted towards a number of rounded bands passing between the uterine surface of the placenta, and the inner surface of the uterus. These bands were generally observed to become elongated, thinner, and of a cellular appearance when put upon the stretch, and were easily torn across; while at other times, though much more rarely, they could be drawn out in the form of tufts from the mouths of the uterine sinuses. On slitting up some of the uterine sinuses with the scissors, these tufts could be seen ramifying in their interior, and were more or less elongated; many of them appearing only to dip into the open mouths of the sinuses, while others proceeded from a quarter of an inch to an inch from the open mouths of the sinuses by which they had entered, and in some cases they extended themselves into one of the neighbouring sinuses. (Plate I. Fig. 1, 2.) The next point was to endeavour to ascertain the nature of these tufts by injection and microscopic examination. A size injection was thrown into the umbilical vein, and though it ran imperfectly from the injury done to the detached portions of the placenta while tracing the course of the utero-placental vessels, yet several of the branches of the tufts contained in the uterine sinuses were filled with injection, and their continuity with the umbilical placental vessels was clearly ascertained. On placing portions of these tufts under the microscope, along with portions of the umbilical vessels taken from different parts of the interior of the placenta, their identity was at once apparent. Having thus determined that these tufts observed in the uterine sinuses of the mother were prolongations of the fœtal placental vessels, I then proceeded to examine their anatomical relations to these sinuses. These tufts were found to protrude into the open mouths of certain of the uterine sinuses only, and it need scarcely be added that they were observed only in those sinuses placed next the inner surface of the uterus, and not in any of the deeper sinuses. These tufts were surrounded externally by a soft tube similar to the soft wall of the utero-placental vessels, which passed between the margin of the open mouths of the uterine sinuses, and the edges of the orifices in the decidua through which the tufts protruded themselves into the sinuses. The size of these tufts varied considerably. Some of them appeared to fill up completely the open mouths of the sinuses by which they entered; while others filled them only partially.* On examining these tufts as they lay in the sinuses, it was evident, that, though they were so far loose and could be floated about, yet they were bound down firmly at various points by reflections of the inner coat of the venous system of the mother upon their outer surface. This reflection of the inner coat of the uterine sinuses upon the tufts was sometimes observed at the point where these entered the open mouths of the sinuses, at other times it was at or near their apices, and was in general so strong that the tufts were torn across in attempting to detach them by pulling. In this uterus we thus ascertained that while some of the utero-placental veins contained no prolongations of the fœtal placental vessels, in others these passed along their interior and projected into the uterine sinuses. On tracing those utero-placental veins, which contained no fœtal vessels, as far as the placental surface of the decidua, the inner coat of the venous system was seen to be prolonged upon some of the tufts of fœtal-placental vessels in their immediate neighbourhood. On tracing one of the larger of the curling arteries through the decidua, it was also observed, that when it reached the placental surface of that membrane, the inner coat of the arterial system of the mother was prolonged upon some of the tufts of the fœtal placental vessels, which projected into their orifices. Those numerous branches of the fœtal placental vessels, which reach the placental surface of the decidua and do not pass into the uterine sinuses, nor into the orifices of the utero-placental vessels, are attached by their apices to the placental surface of that membrane.

---

[*] Lancet, 15th June 1833.

[†] De Organis quae Respirationi Fœtus Mammalium inserviunt; Prolusio Academica. Hafniae, 1837. Vide Wagner's Lehrbuch der Physiologie, Erste Abtheilung, S. 123, 1839.

[‡] Palmer's edition of John Hunter's Works, Vol. iv. p. 67. Mr Owen in his first investigations into this subject adopted the conclusions of Dr Lee, but more lately he has seen good grounds to return to the Hunterian view.

[§] Lancet, 22d June 1833.

---

* The mouths of the sinuses form rounded openings, which are much smaller than the interior of the sinuses into which they open.

On placing some of the filaments, composing these tufts of fœtal placental vessels found in some of the uterine sinuses of the mother, under the microscope, they were observed to divide and subdivide into branches more or less elongated, all of which terminated in blunt extremities. Fig. 3 is an exact representation of a branch of one of these tufts as it was observed through the microscope, and was taken by my friend, Mr John Goodsir, of whose very valuable testimony I have availed myself in almost all the observations I have made on this subject. Few, however, of these branches afforded so favourable an opportunity of observing their mode of termination, for they are almost always more elongated and tortuous than what is here represented, and, from being interlaced, it is generally impossible to unravel them without tearing or otherwise altering their form. A similar examination of the vessels composing the mass of the placenta showed that these presented the same anatomical characters, though, from the intricate manner in which their branches are there interlaced, without, however, having any actual attachment to each other, as they cross and recross each other, there would have been much difficulty in ascertaining this, had those found in the uterine sinuses not been first examined, since they are there in general much shorter and less interwoven than in the placenta itself.* It would be exceedingly difficult to give any exact representation or description of the arrangement of the fœtal placental vessels, for the subdivisions of the larger branches are so numerous, vary so much in length and in tortuosity, and the only point which we are anxious to establish at present is, that, notwithstanding those varieties in the subdivision of their branches, they are all found to terminate in blunt extremities. There is no cellular nor any other tissue filling up the intervals left between the branches of the fœtal placental vessels; and the difficulty experienced in unravelling them does not arise from the presence of any connecting medium, but from the crossing and recrossing of the branches of different tufts, and those of the same tuft with each other. The outer surface of the placental vessels has a smooth appearance, and they are, *we may suppose*, everywhere enveloped in the inner coat of the vascular system of the mother, which, as we have seen above, is reflected upon them.

---

* From very numerous microscopic examinations of the vessels of the placenta, I have no hesitation in saying that the representation of these vessels given in Wagner's Icones Physiologicæ, Tab. xi. Fig. 2, and stated to have been furnished him by Weber, is far from being correct. The placental vessels have an evident resemblance to the representations of the branches of the shaggy corion, given by Raspail, (Organic Chemistry, Plate xiii. Fig. 9) : by Ritgen (Beiträge zur Aufhellung der Verbindung der Menschlichen Frucht mit dem Fruchthälter, Tab. iii. Fig. 5 and 6, 1835,) and by Seiler, (Die Gebärmutter und das Ei des Menchen, Tab. xi. 1832.)

As the blood-vessels forming the placenta, and also those prolonged from it into the uterine sinuses, divide and subdivide into numerous branches, which do not anastomose with each other, but all terminate in blunt extremities, we might on theoretical grounds, as well as from the descriptions given by others, decide that in each of these branches an artery and a vein are bound up together. This conclusion was fully confirmed by the microscopic examination of some of the injected branches of the tufts of the placenta which passed into the uterine sinuses, and also of three other placentæ, in which the arteries and veins were filled with differently coloured size injection. I was satisfied that each of the smaller branches of the placental arteries is bound up with another branch of one of the placental veins, which closely accompanies it, forming what appears a single vessel when viewed through the microscope. Each branch of the umbilical artery is thus closely bound up with a branch of the umbilical veins, and both of them divide and subdivide exactly in the same manner, and terminate in what appears to be blunt extremities, but which actually form the termination of the arteries, and the commencement of the veins, Fig. 4. The interior of the placenta is thus composed of numerous trunks and branches, (each including an artery and an accompanying vein,) every one of which, we believe, is closely ensheathed in prolongations of the inner coat of the vascular system of the mother, *or at least in a membrane continuous with it.* If we adopt this view of the structure of the placenta, the inner coat of the vascular system of the mother is prolonged over each individual tuft, so that when the blood of the mother flows into the placenta through the curling arteries of the uterus, it passes into a large sac formed by the inner coat of the vascular system of the mother, which is intersected in many thousands of different directions, by the placental tufts projecting into it like fringes, and pushing its thin wall before them in the form of sheaths, which closely envelope both the trunk and each individual branch composing these tufts. From this sac the maternal blood is returned by the utero-placental veins without having been extravasated, or without having left her own system of vessels. Vide Plate I. Fig. 5. Into this sac in the placenta containing the blood of the mother, the tufts of the placenta hang like the branchial vessels of certain aquatic animals, to which they have a marked analogy. (Fig. 6.) This sac is protected and strengthened on the fœtal surface of the placenta by *the chorion;* on the uterine surface by the decidua *vera*, and on the edges or margin by the *decidua reflexa*.* The blood of the mother contained in this placental sac, and the blood of the fœtus contained in the umbilical vessels, can readily act and react upon each other through the spongy and cellular walls of the placental vessels, and the thin sac ensheathing them, in the same manner as the blood in the branchial vessels of aquatic animals is acted upon by the water in which they float. According to this view of the structure of the placenta, the fœtal and maternal portions of the placenta are everywhere intimately intermixed, and we find tufts of minute placental vessels with their blunt terminations, lying immediately under the corion covering its fœtal surface, as well as towards its uterine surface. When the fissures dividing the placenta into lobes are so deep as to intersect the whole thickness of the placenta, we may have two or more of these sacs instead of one. The discovery of the prolongations of the fœtal placental vessels into some of the uterine sinuses is principally valuable, as it presents us with a kind of miniature representation of the whole structure of the placenta, Fig. 1 *a*, and enables us to comprehend it readily, for we have there the fœtal blood-vessels, resembling branchial vessels, ensheathed in the inner coat of the vascular system of the mother, and bathed in the maternal blood. The placenta is therefore not analogous in its structure to the lungs, but to the branchial apparatus of certain aquatic animals.

It may at first appear remarkable, that, if tufts of the placental vessels are prolonged into the uterine sinuses, these are not observed projecting from the uterine surface of the placenta when it has been expelled from the uterus in an accouchement. This, however, is explained by the fact, of their being so strongly bound down by the reflection of the inner coat of the uterine sinuses, that they are torn across. As hæmorrhage does not occur from vessels of this size when torn across, we can also explain how fine injections thrown into the placental vessels do not flow from their broken extremities, and how, in certain cases where the placenta and child have been expelled together, and where the circulation through the placenta was allowed to proceed for some time, the blood did not escape at its uterine surface. In a uterus obtained from a woman who died 24 hours after delivery, and which I had an opportunity of examining through the kindness of Professor Simpson, I observed that, while part of the mouths of the uterine sinuses were blocked up with coagula, a considerable number of them were empty. In those which were empty I could detect no tufts, while in those filled up with the coagula I distinctly observed several tufts of placental vessels enveloped in the coagula, when they were placed under the microscope and broken up.

---

* We have no hesitation in following William Hunter in regarding the membrane on the uterine surface of the placenta as the *decidua vera*, notwithstanding all the assertions by Velpeau to the contrary. We have in our possession a preparation of a uterus at the fifth week of utero-gestation, where a thick layer of the *decidua* evidently intervenes between the shaggy corion surrounding the ovum, and the inner surface of the uterus; and it was also observed that some of the tufts of the shaggy corion had insinuated themselves into openings in the decidua.

In detaching the placenta from the uterus, the strongest points of adhesion were found at the place where the tufts were bound down by the reflection of the inner coat of the uterine sinuses, and it is possible that, in some cases, unnatural adhesions of the placenta may be caused by these tufts undergoing the cartilaginous degeneration, and not only be rendered stronger themselves, but also cause a thickening of the membrane reflected upon them.*

I regret that I have not had an opportunity of verifying these observations upon other impregnated uteri, for in the various quarters in which I have made inquiries after such preparations, I have hitherto been unsuccessful in procuring one.†

It appears to have been a common theoretical opinion, during the first half of the 18th century, that the fœtal placental vessels passed through apertures in the membrane placed on the uterine surface of the placenta (then generally termed chorion), and fixed themselves in orifices in the inner wall of the uterus. Brendel ‡ describes the radicles of the chorion as implanting themselves in the inner surface of the uterus, in the same manner as a plant fixes itself in the soil, and in this manner forms blood-vessels which take up a nutritious lymph, which they receive from the maternal vessels of the uterus. Trew§ also describes the filaments of the placenta as proceeding into the substance of the uterus. Rouhault ‖ describes the membrane on the uterine surface of the placenta as "percée dans toute son étendue pour laisser passer les racines des vaisseaux ombilicaux, qui vont ou qui viennent de toutes les parties de la matrice par un nombre innombrable de petits trous." That this description does not refer to the fœtal placental vessels passing into the uterine sinuses which we have above described, but to certain imaginary vessels, is still further evidenced by the statements contained in another paper by the same author, where the membrane on the uterine surface of the placenta is said to be

---

[*] In a placenta containing some cartilaginous masses, into which I had thrown a fine injection, I observed on unravelling one of these after it had been macerated for some days in water, that the injection had only entered the larger trunks of the vessels, and that all the smaller ones were impermeable, and had the colour and consistence of the cartilaginous mass itself, which was in fact entirely composed of them.

[†] It is quite possible that these tufts do not generally project so far into the interior of the uterine sinuses as in the uterus we have examined.

[‡] De Nutritione Fœtus in Utero, published in 1704, reprinted in Haller's Disput. Anatom. Select. Tom. v. p. 488.

[§] De Chylosi Fœtus in Utero, published in 1715, reprinted in Haller's Disput. Anat. Tom. v. p. 445.

[‖] Memoires de L'Academie Royale des Sciences for 1716, p. 343.

pierced by a *million* of small holes, each of which is filled up by as many small capillary blood-vessels.[*] Vater,[†] proceeding on the supposition which we have seen to be common at the time he lived, viz. that the ovum, after exhausting the nutriment which it found in the uterus, sends the filaments of the chorion into the substance of the uterus as a plant shoots its roots into the soil, and being also aware of the existence of the open mouths of the uterine sinuses, states, that the vascular extremities of the chorion insinuate themselves into the *open mouths of the uterine sinuses*, and draw nourishment from them. We find no evidence which would lead us to believe that this description given by Vater was not a mere expression of a theoretical opinion. Noortwyk[‡] describes numerous vessels emerging from the uterine surface of the placenta, inserting themselves into the uterine pores, and forming a manifest communication between the blood-vessels of the mother and fœtus. He afterwards states that these vessels are not continuations of the capillary vessels of the placenta, but vessels "omnino sui generis."[§] Monro *primus*,[‖] after maintaining that the placenta is covered on the uterine surface by a fine membranous continuation of the chorion, states, that "the extremities of the umbilical vessels pierce this membrane, and show their very small orifices on the side next to the uterus, and therefore it is compared to the villous coat of the intestines." J. G. D. Michaelis[¶] describes the fœtal placental vessels penetrating the decidua, and passing into the walls of the uterus. He further adds, that some of them "ad vasorum uterinorum *orificia lateralia quasi agglutinatæ* firmiter sese applicant." We have thus seen that a theoretical notion prevailed for a considerable time, that the fœtal vessels passed through minute and numerous orifices in the decidua; that it was then also generally believed that they entered into small openings in the inner surface of the uterus; and that one author made the fortunate guess that they passed into the open mouths of the uterine sinuses.[**]

It will be at once apparent to those who are familiar with the subject, that the view here given of the anatomical relations of the blood-vessels of the mother to those of the fœtus is considerably different from that proposed by the Hunters, and by those who have succeeded them. The Hunters were not aware that the fœtal placental vessels are prolonged into the uterine sinuses of the mother;[*] and they believed that the intervals between the fœtal placental vessels were filled up by a cellular tissue, into which the maternal blood is poured,—an opinion which has been adopted by many on their authority, and has lately received the sanction of Mr Owen.[†] This cellular structure of the placenta is described by William Hunter as being formed by processes of the decidua, prolonged everywhere in the substance of the placenta, "shooting out into innumerable floating processes and rugæ, with the most irregular and most minutely subdivided cavities between them that can be conceived;"[‡] and he further adds, that the decidua constitutes the uterine portion of the placenta, and forms a number of cells like the *corpora cavernosa penis*, communicating freely with each other.[§] Eschricht[‖] supposes that the utero-placental vessels divide and subdivide in the placenta like the arteries and veins in other parts of the body. The description which approaches nearest to ours is that given by Weber,[¶] but they differ in what I conceive to constitute some of its leading peculiarities. Weber denies that the fœtal placental vessels extend beyond the decidua.[**] He states that the inner coat of the venous system of the mother is prolonged into the placenta, but he describes it as ramifying in the intervals of the placental tufts, in the form of large venous sinuses, upon the walls of which the placental tufts are not only ramified, but also project into their interior, carrying the walls of the sinuses before them. He compares the bronchial tubes of the lungs to the ramifications of the utero-placental vessels in the placenta, and those of the fœtal placental vessels to the pulmonary vessels, with this difference, that the utero-placental vessels do not subdivide into numerous small branches like the bronchial tubes, but form large and wide canals.[††] Wagner in a late work appears to adopt Weber's views, and describes the utero-placental blood-vessels as winding in an expanded net-work around the tufts of the chorion containing the vessels of the embryo, and this net-work, he says, is formed of peculiar but very delicate tubes, of large calibre, especially in the case of the veins.[‡‡]

---

[*] Idem liber, 1714, p. 182.

[†] De Utero Gravido, published in 1725, Haller's Disput. Anat. Tom. v. p. 260.

[‡] Uteri Humani Gravidi Anatome, &c. p. 10, 1743.

[§] Idem liber, p. 14.

[‖] Edinburgh Medical Essays, Vol. ii. p. 102, 1747.

[¶] Dissertatio Inauguralis, Med. De Placenta Humana. The date of this dissertation was obliterated in the only copy which I have seen, but the author was born in 1756.

[**] It is quite possible that this idea may have been repeated by other authors, of whose works I am ignorant.

Note.—The transverse sections of the uterus and placenta in my possession have been examined by many who have paid attention to this subject, among whom I may mention Professors Alison, Allen Thomson, and J. Y. Simpson; and they have all expressed themselves satisfied that the placental tufts were prolonged into the uterine sinuses, and that the inner coat of the veins was prolonged upon them. I have just learned from Professor Sharpey, who was so kind as to examine, at my request, two preparations of impregnated uteri in his possession, that in one of these, which had apparently arrived at the full period of utero-gestation, and was in a condition more favourable for examination, he distinctly observed the placental tufts projecting into the mouths of the uterine sinuses.

---

[*] "It is evident that the umbilical injected vessels do not reach even the outer surface of the placenta, but are only seen through a membrane (decidua) which covers all that surface." William Hunter, opus cit. p. 42.

[†] Opus cit. p. 69.

[‡] Opus cit. p. 43.

[§] This description of the maternal part of the placenta appears to have been adopted by Meckel, (Manuel d'Anatomie Gen. et Descrip. Tome iii. p. 764), Burns, (Principles of Midwifery, 7th edition, p. 189), and many others.

[‖] Opus cit.

[¶] Hildebrandt's Anatomie des Menchen. Vierter Band, S. 495.

[**] "Der Uterustheil ist der an dem Mutterkucken anliegende Theil der Tunica Decidua Vera, in welchen sich die Flocken des Fötustheils der Placenta nicht enstrechen." S. 495.

[††] Opus cit. p. 500.

[‡‡] Lehrbuch der Physiologie, Erste Abtheilung, S. 123. 1839.

Explanation of Figures, Plate I.

Fig. 1. *Diagrammatic sketch of a transverse section of the uterus and placenta.* *a* and *b.* Uterine sinuses, with tufts of fœtal placental vessels prolonged into them. *c.* A curling artery passing through the *decidua vera.* *c. Decidua vera.* *e.* Tufts of placental vessels.

Fig. 2. *Actual representation of a uterine sinus containing tufts of fœtal placental vessels.* *a. Membrana decidua vera* turned up from the inner surface of the uterus. *c.* Walls of uterus. *b.* Tufts of placental vessels in uterine sinus. *d.* Walls of uterine sinus which have been cut through and laid back. *e.* Opening in the decidua, through which the placental vessels passed into the sinus. *g.* Opening through which other tufts passed into the same sinus.

Fig. 3. Actual representation of one of the branches of a tuft removed from one of the sinuses, as seen through the simple microscope.

Fig. 4. *Artery and vein included in each branch of a tuft.* *a.* Artery. *b.* Vein.

Fig. 5. *Diagram to illustrate the views here given of the structure of the placenta.* *a.* Curling artery. *b.* Uterine vein. *c.* Placenta. *d.* Placental tufts, with inner coat of vascular system of the mother enveloping them.

Fig. 6. *Illustration of the branchial vessels of some aquatic animals.* *a.* Artery. *b.* Vein.

Art. II.—*On some points in the Anatomy of the Medulla Oblongata.* By John Reid, M. D. F. R. C. P. E., &c. &c.

Since the brilliant discoveries of Sir Charles Bell have disclosed to us that the motor nerves are attached to the anterior portion, and the sensiferous nerves to the posterior portion of the spinal chord, the anatomical relations of the different tracts of this division of the central organs of the nervous system has assumed an interest and importance to which it had hitherto been a stranger. If we adopt, however, any of the views of the anatomical relations of the different tracts of the spinal chord, as they pass through the *medulla oblongata*, which are given in the anatomical works of this country, there appears to be some blending of the motor and sensitive tracts at that part which cannot be explained; for, while the hypoglossal and abducens nerves arise in the line of the anterior roots of the spinal nerves, we find other motor nerves, such as the *portio dura*, the motor part of the fifth pair, the trochleator, some of the filaments of the *par vagum*, and the spinal accessory, all arising from points posterior to this, and more in the line of the origin of the posterior than of the anterior roots of the spinal nerves.

Fig. 4.

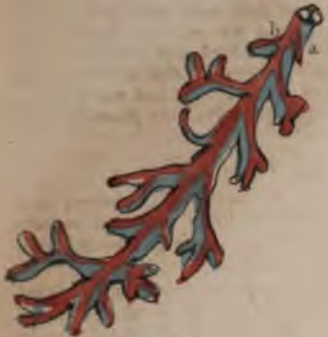


Fig. 3.

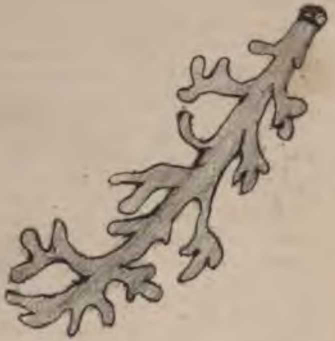


Fig. 6

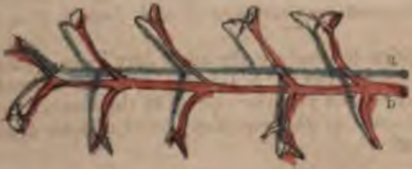
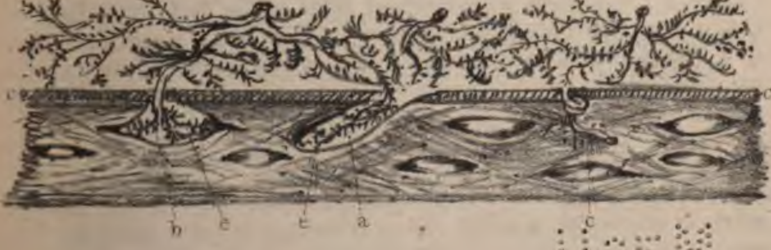


Fig. 1.



UofM

Fig. 5

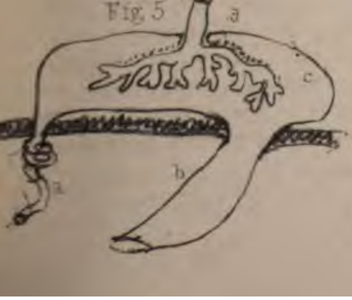
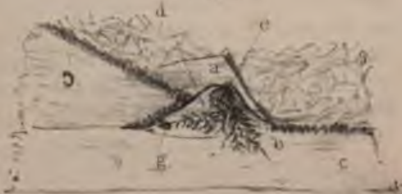


Fig. 2.



The following remarks contain an attempt, founded on repeated anatomical examination, to solve this difficulty. In speaking of the different columns or tracts of the spinal chord, we shall first consider each symmetrical half as consisting of four different columns. There are, 1. the *pyramidal* column, or that connected with the pyramidal body; 2. the *olivary* column, or that connected with the olivary body; 3. the *restiform* column, or that column occupying the lateral surface of the spinal chord between the lateral grooves to which the anterior and posterior roots of the spinal nerves are attached, and after forming the greater part of the restiform body, proceeds upward through the *pons Varolii* to the *crus cerebri*; 4. the *cerebellar* or *posterior* column placed between the posterior lateral groove to which the posterior roots of the spinal nerves are attached, and the posterior longitudinal groove, and which forms the posterior pyramidal body, and posterior part of the restiform body, and passes entirely into the *crus cerebelli.* We shall also speak of each symmetrical half of the spinal chord, as being divided into three columns, as this is a division frequently adopted. Under the *anterior* column, we shall include both the pyramidal and olivary columns; the restiform will be the *middle* or *lateral* column; and the cerebellar will be the *posterior* column. Though the position of the anterior and posterior peaks of the cineritious matter of the chord may serve as a good anatomical division of the spinal chord through a great part of its extent, the anterior peak corresponding to the attachment of the anterior roots, and the posterior peak to the posterior roots of the spinal nerves; yet, as we approach the *medulla oblongata*, it begins to fail us, and we there find the anterior peaks of the gray matter project decidedly in front of the origin of the anterior roots of the spinal nerves.

On tracing the pyramidal columns from the lower margin of the *pons Varolii* downwards to the well known *decussation* at the lower end of the pyramidal bodies, the greater part of the fibres composing these columns arrange themselves into two or more bundles, which decussate or cross each other. These decussating fibres cross the bottom of the anterior longitudinal fissure, and proceed downwards and backwards to join the posterior part of the restiform or middle columns, which they accompany in their course downwards, (Plate II. Fig. 1 and 2.) Every one may readily satisfy himself that none of these decussating fibres proceed into the anterior column of the opposite side, but follow the course we have mentioned, by making a slightly oblique division of the spinal chord at the point of decussation, or by tearing down the pyramidal columns in brains rendered firm by immersion in alcohol, and carefully observing the course which they take. While the greater part of the fibres of the pyramidal columns decussate in the manner we have mentioned, a much smaller number of the external fibres of each column do not decussate, but proceed downwards on the same side, and, along with the olivary column, form the anterior column of that side of the spinal chord. In a small number of the numerous dissections we have made, the whole of the pyramidal column appeared to pass into the middle column of the opposite side. A band of the fibres of the pyramidal column, which in some cases are very distinct, curve round the lower part of the olivary body of the same side, and then proceed upwards and backwards to join the posterior or cerebellar column, as it passes into the *crus cerebelli*, (Fig. 4.) This band of fibres, figured by Santorini, and Gall and Spurzheim, described by Rosenthal,* Rolando, and more lately by Mr Solly, has been termed the *arciform* filaments or band. The filaments forming this arciform band do not decussate. In a preparation in my possession, (Fig. 4,) in which this arciform band is unusually strong, it is distinctly observed to divide itself into two parts, one of these, the larger, follows its usual course to join the posterior column, the other throws itself into the olivary column, (Fig. 4, *b*.) and proceeds downwards along with it. On tracing the olivary columns downwards, they are observed to converge at the lower part of the pyramidal bodies, and below the decussation they are only separated from each other by the longitudinal fissure, and the small band of the pyramidal column which proceeds downwards without decussation, (Fig. 5.) At the upper part of the *medulla oblongata*, the olivary bodies are thus thrown apart by the thick bands of which the pyramidal columns consist at that part; while below the decussation the greater part of the pyramidal bodies have now passed backwards to join the middle or restiform columns, and the olivary columns are consequently thrown forwards, and compose the greater part of the anterior columns. On tracing the olivary column upwards, we observe that, after having surrounded the olivary body which lies among its fibres like a kernel, it passes into the *pons Varolii*, and there divides itself into two bands, which diverge from each other, one of them proceeding upwards and forwards to join the *crus cerebri*, the other proceeding upwards and backwards to reach the *corpora quadrigemina* or optic lobes, (Fig. 6.) † On scraping off the cineritious matter from the floor of the fourth ventricle, and drawing the upper part of the posterior column aside by widening the posterior longitudinal groove, or, what will answer better, by detaching one of the posterior columns at this part, (Fig. 3,) we obtain a posterior view of the decussating fibres of the pyramidal columns already described, as they pass into the posterior part of the middle columns. If we were restricting our examination to this surface alone, we might be led to believe that the decussating fibre brought into view formed a decussation between the two middle columns, and it has been described as such.* No doubt, if we describe the middle column as proceeding from below upwards, as is now most frequently done, it may be justly said that this is a decussation of part of the fibres of the middle column; but then it ought to be added, that these decussating fibres do not pass into the middle column of the opposite side, but into the pyramidal column of the opposite side. After the most careful search, we have not been able to find any other decussation besides that we have mentioned. With these remarks upon the anatomy of the *medulla oblongata*, we now proceed to point out the attachments of the various nerves to the different columns. We have stated above, that the olivary columns converge towards each other at the lower part of the pyramidal bodies, in consequence of the greater part of the pyramidal columns passing backwards to join the middle lateral columns. If we trace these olivary columns downwards, we observe that they afford attachments to the anterior roots of the first and second cervical nerves, (Fig. 5,) and that they continue their course onwards in the line of the other anterior roots of the spinal nerves. We can then have little difficulty in arriving at the conclusion, that the olivary is a motor column. On tracing this column upwards, (Fig. 6,) we find that where it embraces the olivary body, the *portio dura* is attached to its outer margin, and the hypoglossal and abducens nerves are partly attached to its inner margin, and partly to the outer margin of the pyramidal column. On tracing the two roots of the fifth pair, the smaller or motor root can be followed to that portion of the olivary column which proceeds to the optic lobes,—sometimes running down the outer or external edge of the pons to reach the portion of the olivary column already mentioned, as it immerges from the external margin of the pons, at other times its course is obscured by a greater or less number of the transverse fibres of the pons crossing it. The trochleator nerve is attached to the internal margin of the same band of fibres when it has ascended the *processus a cerebello ad testes*, and is about to enter the optic lobes. The larger or sensiferous root of the fifth, on the other hand, proceeds downwards, and passes beneath a thick mass of the transverse fibres of the pons to reach the middle or restiform 'column, to which it is undoubtedly attached ; and to this same column the whole of the fibres of the glosso-pharyngeal, and nearly all the fibres of the *par vagum* are attached. In those cases where the arciform filaments are well marked, (Fig. 4,) we have observed a few of the filaments of the *par vagum* attached to them as they cross the middle column, and it is probable that the few motor filaments contained in this nerve are derived from this source. The filaments of the *par vagum* are certainly not connected with the olivary bodies, and these cannot be the ganglia of these nerves, as Mr Solly has supposed.*

---

* Ein Beitrag zur Encephalatomie, S. 28. Rosenthal describes this as a marked medullary layer passing from the pyramidal bodies over the inferior parts of the olivary bodies to the *posterior column*, and thus forming a strong bond of union to these parts, which varies in strength and thickness in different cases.

† Rolando and Mr Solly (The Human Brain, &c. p. 148–49,) deny the existence of this olivary tract; but this appears to us a point about which no doubt can be entertained.

---

* The Nervous System of the Human Body. By Sir Charles Bell, p. 218, 1836.

With regard to the origin of the spinal accessory nerves, I find it a more difficult matter to speak with precision ; but there can be no doubt that the decussating fibres between the pyramidal and middle columns run down in the course of the spinal accessory, and we can easily imagine how the filaments of this nerve could reach these decussating fibres.

It would be an interesting point to ascertain the functions of these decussating fibres, but this, from their position in the cranium, would be a matter of great difficulty. Their connection with the root of the spinal nerves is not sufficiently obvious to enable us to draw any decisive conclusions regarding their function ; and though we have described them as proceeding from the pyramidal to the middle columns, yet we may perhaps with equal justice describe them as proceeding from the middle to the pyramidal columns. In two kittens deprived of voluntary motion and sensation by a dose of prussic acid, I observed extensive muscular movements on irritating the upper part of the pyramidal bodies with the point of a needle ; but whether these belonged to the class of excito-motory movements of Dr Marshall Hall, or whether they resulted from the impression being conveyed outwards directly to the muscles moved, I could not determine. If it should be afterwards ascertained that this is an excito-motory column, it might open up a new field of investigation.

Though the decussation at the lower part of the pyramidal bodies in even all the late anatomical works of this country is still considered as a decussation merely between the two anterior columns, yet it appears that it was correctly described by Fr. Rosen-

---

* Opus cit. 146–47–48. Mr Solly adduces in favour of this view the "amazingly developed" *corpora olivaria* of the porpoise, (p. 101.) In this, however, we very strongly suspect he is mistaken, for while dissecting the brain of a porpoise, we carefully looked for these large *corpora olivaria*, and could not observe them. We have also heard urged in favour of this view, the statement made by Tiedemann in his work on the fœtal brain, that the *corpora olivaria* are imperfectly developed at the sixth month,—a period at which there is no use for these nerves. Whether this argument ought to be considered legitimate or not, we need not inquire ; but we have two preparations in our possession procured from fœtuses, where we had undoubted evidence that neither of them had passed the sixth month of utero-gestation, and in both, the *corpora olivaria* are very prominent, and relatively as well developed as in the adult.

PLATE II.

Fig. 1

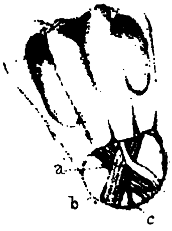


Fig. 3

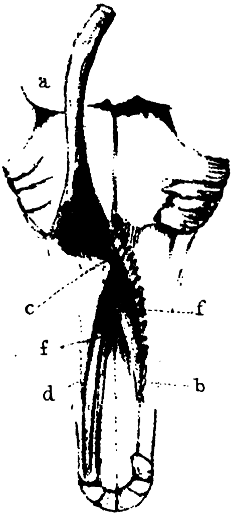


Fig. 2

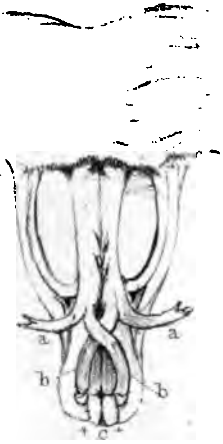


Fig. 5

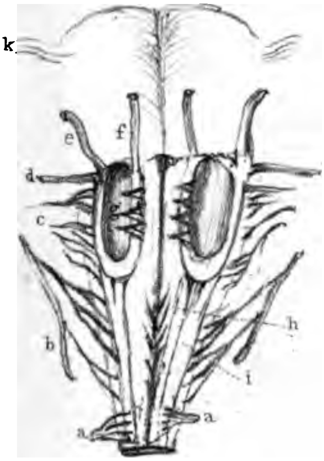


Fig. 4

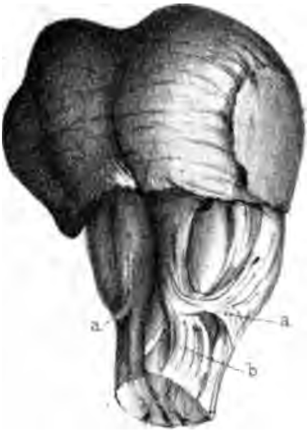
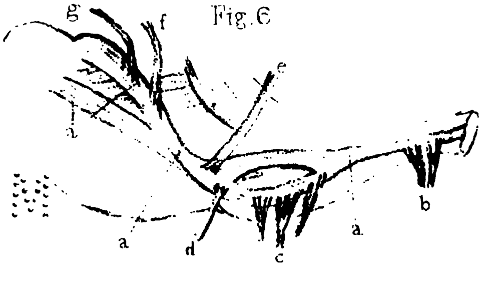


Fig. 6



Edinr M[illegible] Surg. J[illegible] Dr HOLLAND'S PAPER. Plate III. p. 17. Vol. LV.

Fig. 1.

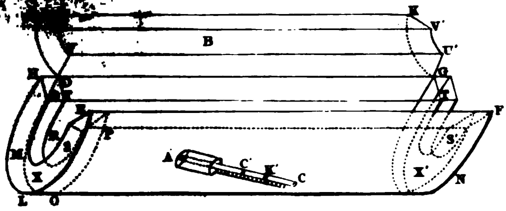


Fig. 2.

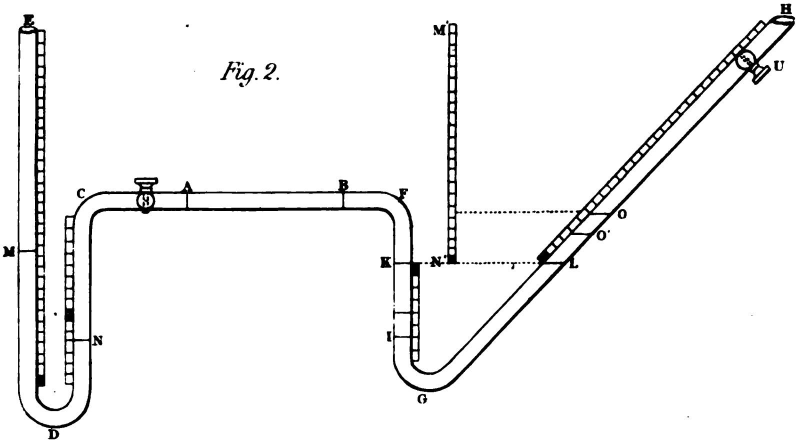


Fig. 3.

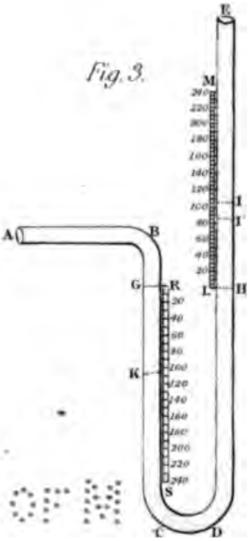
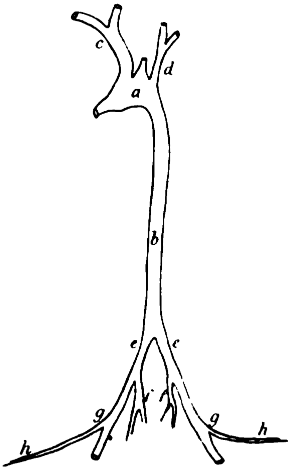


Fig. 4.



G. Aikman, Sc.

thal in 1815.[*] He states that the pyramidal bodies ascend as a small column at the sides of the anterior fissure of the *medulla spinalis ;* that they become thicker and broader at the extremity of the olivary bodies ; and that " the sudden increase of its fibres is caused by the accession of small bundles which arise deeply from the large posterior fasciculus of the great column. It is these small bundles which decussating become interwoven with each other, so that several of those of the left side actually pass into the fibres of the right pyramidal body, and *vice versa.*" Cruveilhier[†] states, that the anterior pyramidal bodies are certainly not formed by a continuation of the anterior columns of the spinal chord, but by fibres from the lateral columns. Arnold's[‡] description of the decussating fibres does not differ essentially from that of Rosenthal.

Explanations of Figures, Plate II.

Fig. 1. *Oblique section of spinal chord at the decussation of the pyramidal columns.* *a.* decussating fibres. *b.* posterior column. *c.* middle column.

Fig. 2. *Anterior view of decussation.* *a.* external fibres of pyramidal column detached and thrown outwards. *b.* decussating fibres running into posterior part of middle column. *c.* posterior columns.

Fig. 3. *Posterior view of decussation.* *a.* posterior column of one side detached and thrown upwards. *b.* posterior column of opposite side in its natural position. *c.* mass of cineritious matter. *f. f.* decussating fibres.

Fig. 4. *a. a.* Arciform filaments of medulla oblongata.

Fig. 5. *a. a.* Anterior roots of spinal nerves. *b.* spinal accessory nerve. *c.* par vagum. *d.* glosso-pharyngeal. *e.* portio dura. *f.* abducens oculi. *g.* hypoglossal. *h.* 5th pair. *i.* olivary column. *k.* decussation of pyramidal columns.

Fig. 6. *a. a. a.* Olivary column. *b* anterior root of first spinal nerve. *c.* hypoglossal nerve. *d.* abducens nerve. *e.* portio dura. *f.* smaller root or motor part of the fifth pair. *g.* trochleator nerve.

ART. III.—*The properties and influence of Arteries on the Circulation of Blood.* By G. CALVERT HOLLAND, M. D. Physician to the Sheffield General Infirmary.

To endeavour to reconcile the discrepant opinions of physiologists on the structure and properties of the arteries, by appealing to experiments, would be a vain and unprofitable undertaking. The results obtained by different able observers present scarcely any points of agreement. By one, it is contended that the arteries are muscular, and possess considerable contractility; and the opinion is stated to rest on careful examination of their structure, and the effects of the application of external agents. The evidence in favour of the doctrine appears unanswerable. The mind is not called upon to believe anything but what is presented to the senses. Conclusive, however, as the evidence may appear, it is treated by another inquirer as a fiction,—an ingenious device to account for phenomena which may otherwise be explained. In support of his views, he equally appeals to experiments, and calls upon the impartial understanding to examine the results. These, he argues, prove that the arteries are neither muscular nor contractile, but simply endowed with elasticity. The external agents, mechanical and chemical, which the one urges as satisfactorily establishing his doctrine, are asserted by the others to be incapable of producing the several effects contended for. Indeed, the same experiments do not furnish the same phenomena. A third inquirer, founding his faith on experimental researches, simple and easy of application, denies altogether the exercise or modifying influence of either contractility or elasticity on the motion of blood.

---

[*] Ein Beitrag Zur Encephalatomie, Weimar, 1815.—When exhibiting the preparations from which the above description was drawn up, to Dr Sharpey while on a visit to Edinburgh last autumn, he informed me of the nature of Rosenthal's work, and kindly sent me his copy for perusal.

[†] Anatomie Descriptive, Tome iv. p. 594. Paris, 1835.

[‡] Bemerkungen über den Bau des Hirns und Rückenmarks, &c. Zurich, 1838. S. 30. In his Tabulæ Anatomicæ Fasciculus, i. Tab. ix. Fig. 1, he represents the pyramidal bodies as being formed entirely by fibres from the lateral column.

Our business is not to endeavour to reconcile these discordant conclusions, but to examine the numerous facts adduced, as bearing upon or illustrating the phenomena of circulation. The inquiry on which we enter is not biassed by any preconceived opinions of our own, nor have we any particular views to establish, except what arise from the analysis of the subject. We are not the decided advocates or opponents of any of the prevailing doctrines. The attainment of truth is the only object of the present inquiry. A patient examination of the labours of past and contemporary physiologists will in all probability be more useful than any attempt to emulate them by adducing new facts, except such as may be elicited by a critical study of those with which we are familiar. The demand for bold and novel experiments is less urgent than a just appreciation of the nature and bearing of the numerous results, theoretical and practical, which surround and perplex the inquirer. To facilitate their better understanding is the object of our researches.

Magendie, in entering upon the consideration of the properties and influence of the arteries, remarks, " we have applied ourselves chiefly to study the various circumstances under which the elasticity of the arterial parietes is developed, and have shown how this physical property is intimately connected with the most important condition of the circulation in the human body. There is not, in fact, a single phenomenon of any consequence which may not be explained by reference to this principle, and to this only. The motion of the blood through the different tubes, whether arterial or venous; its rapidity in the different sections of each system; the perfect regularity of its progress; the jerking stream which flows from a divided artery;—all these are circumstances, the explanation of which is to be sought for in the physical properties of the vessels themselves, and in the mechanical nature of the force which gives the blood its first impulse.

According to some physiologists, elasticity simply allows an artery to return to its previous state on the cessation of the distending cause; and consequently, the reaction is proportionate to the tension which precedes it. This definition appears philosophical and just. In this restricted sense it is not, however, received by others. To this property Magendie attributes extraordinary influence. According to him, it not only acts in the manner stated, but is further capable of expelling the whole contents of an artery; and experiments are adduced in proof of the fact. The arterial system is regarded as always full of blood, and therefore, the additional quantity transmitted by the contraction of the left ventricle necessarily produces distension. In the interval of two contractions, the arteries act upon their contents, in virtue, it is said by one, of elasticity, and by another, of muscular contractility. Numerous experiments and phenomena are imagined to establish the existence and influence of the former property, but these may be shown to be much less conclusive than they are generally regarded. Bichat and Parry state distinctly that no dilatation of the arterial system was detected on the contraction of the left ventricle, and even Magendie, the great advocate of elasticity, acknowledges that dilatation is perceived only in the aorta. Poiseuille is considered to have proved by experiment the dilatation of arteries, and the force they exert in returning upon their contents. This physiologist is regarded as having thrown considerable light on the phenomena of circulation. His experiments are referred to, as affording decisive evidence of the truth of his opinions. To doubt the accuracy of the results at which he has arrived, which are imagined to possess the certainty of physical researches, may indeed appear bold and presumptuous. The conception and execution of his experiments deserve praise, and far be it from us to withhold what is justly due. Our admiration must not, however, blind us to glaring defects; to serious errors, arising from the application of physical science to the investigation of vital phenomena. Many distinguished physiologists of the present day will unquestionably retard the sound progress of the study of man, by their constant appeal to experiments, and the doctrine inculcated, that the animal machine is comparatively simple, and its important laws explicable on purely mechanical principles. At the head of those who teach this doctrine, and whose labours have largely contributed to its illustration, stands Magendie. His intelligence, enterprize, and tact, give him a marked pre-eminence, but he has travelled at too great a speed, and over too ample a space, to have been able to note or examine fully what was most interesting in his path of inquiry. His progress exhibits considerable industry and varied researches, but few elevations affording a clear and comprehensive view of the vast field of vital phenomena; nor is the mind capable of deducing from the multifarious objects presented principles to any great extent applicable to medicine. The justness of these strictures will be established by a critical analysis of the most important of his labours and contributions to physiology. The experiments of Poiseuille were mostly performed under his superintendence, and furnished the same results when subsequently repeated by him; and therefore, they come before us invested in all the imposing qualities of physical truths.

The instrument employed by Poiseuille to establish the existence of dilatation is described in Plate III.

M N is a tin tube of two decimetres in length (Fig. 1), about 1.37 inch, and thirty-five millimetres in diameter, about 8 inches. On its convex surface is contrived an aperture, D E F G, occupying its whole length, which can be closed by a sort of door D G K I. The extremities M and N, (see M in the figure), present grooves formed by the two plates H L E R Q, and D O P S T, about one centimetre, = 39.100 inch, distant from each other, and so fluted in their centre as to exhibit a segment of a circle of twelve millimetres, = 4.700 inch, in diameter. The door I D G K supports two plates, I D V U, K G U′ V′, whose margins V U, V′ U′ are cut circularly, so that when closed, they meet to form entire circles with the corresponding segments F × ′ S. In the middle of this door, B appears a circular orifice of two centimetres, or 7.110 of an inch in diameter, the use of which will presently be apparent.

At a point A in the surface of the cylinder is an opening of twenty millimetres (78.100 of an inch) in diameter, into which is fitted a plug of cork, receiving a small glass tube, of three millimetres (11.100 of an inch) internal diameter. A scale divided into millimetres (39.1000 of an inch) is there fixed. This tube is nearly horizontal.

The trunk of the carotid of a horse is exposed to the extent of $11\frac{8}{10}$ inches; ligatures being applied to each of the branches arising from it for the purpose of completely isolating the artery. The cylinder is opened, and the exposed portion of the artery placed in it, which, however, still adheres by its extremities to the animal. The door I D G K is closed, and there is dropped into each of the grooves a mixture of suet and wax; the joints of the door are luted; and thus the cavity of the cylinder has no communication with the outside, excepting by the aperture B, and the glass tube A C. By this orifice B some water at about 36° is introduced. A part of this water enters the tube A C, say as far as C′; the cylinder being now filled with water, and no longer containing any air, the orifice B is closed by a plug. Everything being thus arranged, the artery is encased in the cylinder, and the blood moves in its interior, as in the ordinary state. If the tube A C be examined, the water is seen to change its level, and to move from C′ to K′, and *vice versa*, and that too at each contraction of the heart. In the experiment now detailed, the distance between the points C′ and K′ was 2.7 inches; thus the artery being 35.100 of an inch in diameter, its 8 inches of length, in consequence of their dilatation from each contraction of the heart, presented an increase of volume equal to that of a solid cylinder, whose height was C′ K, and the diameter of whose base was that of the small tube A C, viz. three millimetres, (11.100 of an inch.) We may notice that this dilatation is far from being considerable, and not easily recognized by a single observation, even in an artery of the calibre of that experimented on, after being exposed."*

Poiseuille remarks that in this experiment " the artery is encased in the cylinder, and the blood moves in its interior as in the ordinary state."

Were arteries inorganic tubes, his reasoning might be admitted; but certainly not otherwise. He institutes no inquiry to ascertain what is the natural condition of these vessels, but evidently imagines that they are unsusceptible of any serious modification, or incapable of being deranged by any experimental researches. His deductions are founded upon this assumption, and in this lies the great error of his ingeniously contrived experiments. The constrained position and suffering of the animal would greatly modify the conditions of the circulation. The heart would be excited to violent action, and the fluid propelled by it would meet with impediments in its course, which would necessarily produce distension of the artery; and still further, the effect would be aggravated by the application of ligatures to the branches which it gives off. The blood being confined within narrower limits would cause great fulness of the vessel. But, independently of the influence of the latter circumstance, the suffering and position of the animal are quite sufficient to occasion an unusual degree of distension, not only in the artery in question, but in all the principal arterial branches. If the intention of the physiologist had been to produce this effect, or the widest possible departure from the natural condition of the vessel, he could not have succeeded more admirably. With what truth can it therefore be asserted that blood circulates in the artery as under ordinary circumstances? Whenever the respiration is accelerated and disturbed, and especially in experimental researches of this kind, the result here stated invariably follows. What, then, is the legitimate inference deducible from the experiment? That an artery, already round and full, is susceptible of further enlargement. The dispute is not whether arteries have a varying capacity, at one time small, at another large and full to distension, but whether they are dilated in the ordinary conditions of the circulation and to what extent. This experiment furnishes no data to direct and elucidate the inquiry. Without suspecting the possibility of error in these researches he concludes.

---

* Journal de Physiologie, Vol. ix. p. 46.

"The arteries, then, dilate in circulation; but if their volume is increased, their parietes must be distended; and, as their middle coat is highly elastic, in recoiling on themselves they give birth to a force; and it is this force of contraction entirely passive, due to the elasticity of the arterial parietes, brought into play by their dilatation, which we propose at present to estimate."

In the experiments of Barry, the coloured fluid in the instrument employed was observed to ascend towards the chest during inspiration, but when performed on the horse standing, in place of being held down forcibly, this effect was scarcely perceptible. In treating of this subject it was remarked, that phenomena, the result of experiments which create an extreme or unnatural condition of the circulation, cannot certainly be regarded as elucidating the laws by which this important function is carried on, in the usual or undisturbed state of the vital powers. The same remark may, with equal force, be applied to the experiment of Poiseuille. The means employed to determine the elastic properties of arteries, and their influence on the motion of blood, should in no way modify their natural condition. To induce a change is, in all probability, to introduce causes not previously in operation, and consequently to give rise to effects which may greatly mislead the judgment in its calculation and reasoning. This truth is obvious in the subsequent attempts of Poiseuille, to measure the elastic force of arteries, or the amount of their influence on circulation, and to these attempts, which are thus minutely described by him, especial attention is solicited.

"A, B, (Fig. 2), is an arterial tube 250[*] millimetres ($9\frac{8}{10}$ inches) in length, belonging to the trunk of the carotid, which has just been detached from a living horse; its diameter is nine millimetres (35.100 of an inch); it has a horizontal position, and its extremities are tied on to the horizontal branches A C, and B F, of the two glass-tubes A C D E, B F G H, whose branches E D, C D, and F G, are vertical. The branch G H is inclined, and forms with the vertical one an angle of nearly 50 degrees; in the portion K G L of the tube F, K G H, there is a quantity of quicksilver, the levels of which are K and L. The portion L H is filled with water, and there is a stopcock at H, which is closed after the introduction of the water from the level of the mercury at L to the extremity H. This tube B F G H has a small diameter, say of three millimetres (11.100 of an inch). From K towards G there is a scale, the amount of whose divisions correspond with the millimetres of a third vertical scale M′ N′. The portion B F

---

[*] The length of the tube was measured in the living animal before being detached.

K of the tube B F G H, the artery A B, and the tube A C D E, (which has a stopcock at A,) are filled with water. A quantity of mercury being introduced at the orifice E, the water pushed by the mercury from D C A mounts into the artery A B, and communicates to the yielding arterial parietes a pressure increasing in proportion to the quantity of mercury introduced. When a quantity of this metal is introduced sufficient to produce the pressure we wish to obtain, the stopcock at A is shut, and thus all communication between the artery A B, and the tube A C D E is intercepted. By means of the vertical scales placed along the branches D E and C D, we can calculate the pressure to which the parietes of the artery are submitted. M and N are the levels of the mercury in these two branches. In the present experiment, the difference of the levels is equal to 85 millimetres ($3.\frac{44}{100}$ inches); the portion E M, which is filled with water, to 278 millimetres, and C N, also filled with water, to 148 millimetres: we have then the force which dilates the artery expressed by the height of a column of mercury equal to

$$\overset{\text{mill.}}{85} + \frac{\overset{\text{mill.}}{278} - \overset{\text{mill.}}{148}}{13} = \overset{\text{mill.}}{85} + \frac{\overset{\text{mill.}}{130}}{13} = \overset{\text{mill.}}{85} + \overset{\text{mill.}}{10} = \overset{\text{mill.}}{95}$$

(the densities of the water and mercury which we employed were to each other as 1 to 13).

"The stop-cock at H is then opened, and the artery by virtue of its elasticity, suddenly recoils upon itself; the mercury is depressed from K to I, and then rises from L to O, and as we have K I = 63,25 mill.; K F = 50 mill.; P N = 39 mill.; P M = 216,12 mill.; there is obtained the force of contraction of the artery represented by the height of a column of mercury equal to 63,25 mill. + 39 +

$$\frac{\overset{\text{mill.}}{216,12} - (\overset{\text{mill.}}{50} + \overset{\text{mill.}}{63,25})}{13} = \overset{\text{mill.}}{102,25} + \frac{\overset{\text{mill.}}{102,87}}{13} = \overset{\text{mill.}}{102,25} + 7,91 \text{ mill.} = 110,16.$$

"During the whole experiment we must take care to keep the artery in a warm medium, by means of water at 36 degrees. (Cent. = 97° F.). On comparing the present result with the preceding, we find, that the entirely passive contraction of the artery, in consequence of the elasticity of its parietes, brought into play by the dilatation is superior to the force which dilates it."*

This experiment is supposed to prove that the elasticity of an artery is even superior to the force by which it is excited, and, moreover, that it co-operates powerfully with the heart in the propulsion of blood. The experiment will be admitted as establishing the existence of elasticity, but not as determining its degree of influence on the motion of blood. The two questions are very distinct. The one is simple and easy to solve, the other is complex and exceedingly difficult. No experiment can be devised to illustrate more satisfactorily the operation of elasticity, but to determine its agency in the natural conditions of the circulation is perhaps impossible. Suppose the blood move in the arteries in virtue of elasticity and the impulse of the heart, in any experiment instituted to ascertain to what extent the former power co-operates, it is imperatively necessary not to disturb the latter. They have relations to each other which must be scrupulously preserved. The arterial fluid would not be at rest in the interval of the contractions of the heart were elasticity altogether wanting. There are powers by which it would be solicited or urged along its course, and consequently it is impossible to determine the exact influence of elasticity on the circulation. When this property is imagined to be exerted, the blood is in motion, but in the experiment the fluid employed is at rest. To render this or any other experiment of value, the distension of the arterial system in its ordinary condition ought to be known, as a preliminary step, but it unfortunately happens that there are no data to guide the physiologist in such calculation. In the experiment, the artery is distended, but whether to a greater or less extent than natural, is entirely a matter of conjecture. The result cannot, therefore, convey a correct idea of the influence of elasticity on the motion of blood. Its existence is shown, but not the exercise of it under its usual circumstances. The analysis of these experiments will not be without its value, even if the justness of the reasoning be disputed. It points out difficulties inherent in physiological researches, and especially when important organs are rudely injured, or the vital actions of the system generally are disturbed. Experiments cannot dispense with the necessity of deep and laborious thought, nor the greatest possible caution in drawing conclusions. This appealing directly but coarsely to nature is often a fruitful source of error.

---

* Journal de Physiologie, par F. Magendie, Vol. ix. p. 48.

Before proceeding further, we will survey the ground over which we have passed, and place before the mind in few words the objections to these and all similar experiments.

The experiments of Poiseuille have two objects in view. The one is to demonstrate the existence of elasticity, the other to measure its influence. The first experiment is supposed to set at rest the question of dilatation; but under what conditions of the arterial system?

1*stly*, The artery is manifestly in a state of distension, and every fresh quantity of blood transmitted by the left ventricle will increase it. The fluid injected, in pushing the mass before it, will unquestionably produce this effect. The same phenomenon would present itself, were a vein experimented upon under similar circumstances; but it is not possible to place them in analogous conditions.

2*dly*, The arterial fluid is admitted to be constantly in motion.

Were the heart the only propelling power, the blood would not be at rest after every contraction of the left ventricle. It would move forward, and, how trifling soever in degree, this would necessarily be followed by an apparent contraction of the artery, or at least by a diminution of its calibre. It becomes, then, a difficulty to determine, whether this phenomenon is the result of an active vital property in the vessel, or occasioned by the motion of the blood after the contraction of the ventricle. Precisely the same effect would be exhibited by the first instrument in either case: the artery would be distended by the next quantity of blood injected. In analysing the experiments of Magendie on this subject, it will be shown that he also entirely neglects this distinction.

3dly, It is admitted that the dilatation of the artery is demonstrated by this experiment; but this might have been inferred from a knowledge of the circumstances in which the vessel is placed. A vein, were it possible to subject it to analogous conditions, would unquestionably present the same result, and therefore the experiment affords no information respecting the influence of elasticity on the arterial current.

4thly, The object of the second experiment is to measure the force of this property on the motion of the blood. There are no data on which to proceed in this inquiry. The first step is one of conjecture. The distension of an artery, in its ordinary condition, is constantly varying, and at no one moment is it possible to ascertain it. It is an unknown quantity, defying all analysis.

5thly, The experiment proves that an artery, when unnaturally distended, as in this instance, is capable of reacting on its contents, but no inference can be drawn from this fact in regard to the agency of elasticity in the circulation of blood. To assert that this property maintains the same intensity of motion throughout every part of the arterial system, is an assumption, and rests on no probable grounds. The doctrine which the phenomenon is adduced to support will, in subsequent investigations, be shown to be fallacious.

We pass now to the examination of the experiments of Magendie, which are adverted to by all physiological authorities, as proving the extraordinary influence of elasticity on the motion of blood. We give his own words:

" When two ligatures are applied at the same time, and at the distance of one or two inches from each other, to two points of an artery which does not give off any branches, as the carotid, we have a length of artery in which the blood is submitted to the sole influence of the parietes. If a small aperture be made in this portion of the vessel, nearly all the blood which it contained is ejected, and the artery is much contracted on itself. This experiment, which has been long known, never fails, and I cannot imagine on what grounds some writers have allowed themselves to doubt its reality."

Does the blood, in this experiment, flow from every particle being in motion, or from the action of the artery?—The fluid, included by the ligature, was, previous to their application, circulating from the impulse of the heart, and though arrested, its activity cannot at once be suspended. Were the upper ligature removed, the blood would flow in its natural direction, and puncturing the vessel merely allows this tendency to circulate or escape to manifest itself. The structure of an artery does not allow it to contract with force upon its contents. The effect is an impossibility. Were the elasticity of the arterial parietes the cause of the phenomenon, and indeed the only one in operation, its influence would be much more evident than it is in the ordinary conditions of the circulating system. When the arterial fluid is moving forward from the direct impulse of the heart, there is a ready escape for it on the exercise of contractility. The force with which blood flows from a punctured vein might with equal truth be enlisted in corroboration of the same doctrine. The projection of the fluid in this case arises, however, neither from the action of the vessels nor that of the heart, and there is as little evidence that the phenomenon in the experiment of Magendie is attributable to the former of these two powers. Did the atmosphere offer the same amount of resistance to the arterial blood escaping from its vessels, as is experienced by it in its ordinary circumstances by the mass of blood before it, no jet would be perceived, nor would the contents of the included portion of the artery be expelled. To render the experiment satisfactory, the resistance should be equal in both cases. Were it possible to place the artery in *vacuo*, the projection of the fluid would be still greater, but clearly not from the increased action of the arterial parietes; yet this inference might be drawn, as in the experiment in question, and certainly with equal propriety and force. The effect is obvious,—the projection of the fluid; but the cause has been assumed without sufficient consideration of the difficulties attendant on the explanation. Magendie adduces another experiment, and one which is peculiarly his own, in support of the doctrine which we are here combating.

"Let us notice another experiment which seems to me suited to put in its proper light the phenomenon of the contraction of the arteries. I exposed to a certain extent the crural artery and vein of a dog. I passed a ligature beneath these vessels, and afterwards strongly compressed the posterior part of the thigh, so that all the arterial blood arrived at the limb by the artery; and in some cases the vessel completely emptied itself in the portion situated below the ligature. It is therefore evident that the force with which the arteries recoil on themselves is quite sufficient to expel the blood which they contain."

In what manner is the blood circumstanced in the artery previous to the application of the ligature? Is it at rest, or in motion? If in motion, which it is unquestionably, how is it affected by the ligature? This cannot subtract from it the impulses of the heart which it has already received, and therefore, it continues to flow towards the capillaries until these vessels have received the whole contents of the artery. According to this view, the phenomenon does not arise from the contractility of the artery, but from the tendency of the blood to pursue its course, partly in virtue of the propulsive power previously imparted to it. Were contractility the cause, as Magendie and others contend, the artery would not be entirely emptied. Its partial contraction would expel only a small portion, and not the whole of its contents. This fact ought to have awakened suspicion as to the correctness of the view taken. We are further told by the same physiologist, that, if a ligature be tied on the carotid artery, the blood above it is wholly expelled, or if on a vein, the blood below it or nearest the heart, circulates and leaves the vessel empty. Previous to the application of the ligature, the blood in the vein is similarly circumstanced to that in the artery, it is in motion, and whether from a *vis-a-tergo*, or causes acting before it, is immaterial to the argument. The ligature cannot arrest this motion in either vessel. The capillaries, acting in advance of the arterial current, will tend to withdraw the blood from the artery. The tying of a ligature will indeed influence the current: whether an artery or vein be the subject of experiment, the fluid will be urged forward by it in its natural direction. When two ligatures are applied, if the latter be so placed, which it may readily, as to distend greatly the included portion of vessels, its contents will be projected with accelerated force, and certainly not from the exercise of any increased contractility, but simply from the greater tendency of the blood to escape at this time. These experiments are constantly appealed to in proof of the influence of elasticity on the motion of the blood. They cannot, however, if these strictures be well founded, be admitted as establishing the fact.

We pass now to the experiments of Poiseuille, instituted in order to determine the force with which the blood circulates. This had always been regarded as diminishing gradually with the onward progress of the fluid. All physiological authorities were unanimous in this opinion; and the doctrine, indeed, appeared to rest on self-evident data. Poiseuille has, however, recently attempted to prove that the blood is urged forward with as great a momentum in a small artery far from the heart, as in any important branch near it. This extraordinary doctrine comes before us in an exceedingly plausible form,—the result of direct experiments, which have been repeated by Magendie with corresponding effects. The experiments are certainly ingenious and well devised, but may be shown not to warrant the conclusions deduced from them. To prevent misapprehension of the views of Poiseuille on this subject, we give his own words.

"Fig. 3 is a glass-tube presenting a horizontal branch, A B; a vertical descending branch, B C; and an ascending branch, D E; so curved as to exhibit at B the fourth of a circle, and at C D a semicircle. Supposing mercury to be introduced into the portion G C D H, the tube being in a vertical position, the levels of the mercury G and H will be at the same height in both the branches. If the blood enter into the portion A B G by the orifice A, inosculated with an artery, it will press on the surface G of the mercury; the metal will be depressed in the branch B C from G to K, for instance, while it will rise in the branch D E to I. It is evident that, in accordance with hydrostatic laws, the whole force with which the blood throws itself into the artery will be estimated by the weight of a cylinder of mercury, the base of which is a circle, having for its diameter that of the artery, and whose height is the difference J K of the two levels of mercury, deducting the height of the small column of mercury, which may form an equipoise to the column of blood B K."

After this description of the instrument, he proceeds to show how it acts in the experiments in question.

"Meantime let the apparatus be fitted to the carotid artery of a dog; the distance B G, which measures the height of the sub-carbonate of soda above the level of the mercury must be determined. Let us suppose, for greater simplicity, that the portions G K C and H J′ I of the tube are exactly of the same diameter, so that, on observing on one and the same scale, L M, the height H I to which the mercury has risen, we shall see that it is sunk below the point G of the same quantity H I. Thus, in order to find the height of the mercury, owing to the force of the blood, we shall only have to double the height H I, and to subtract from this result the pressure of the mixture of blood and sub-carbonate of soda, owing to the column $B\,C\,K = B\,G + G\,K = + B\,G\,H\,I$.

"Let us take, then, $B\,G = 25$ millimetres, nearly one inch, and $H\,I = 105$ millimetres, or four inches. Let us suppose again, as we have found, that a column of the mixture of blood and subcarbonate of soda of 10 millimetres, or four-tenths of an inch in height, furnishes an equipoise to a height of one millimetre of mercury, we shall have for the pressure sought,

$$105 + 105 - \frac{25 + 105}{10} = 210 - \frac{130}{10} = 210 - 13 = \overset{\text{mill.}}{197}$$

and for a height H I′, equal to 85 millimetres, or 3.34 inches, for instance, the pressure we should have would be

$$85 + 85 - \frac{25 + 85}{10} = 170 - \frac{110}{10} = 170 - 11 = 159 \text{ mill.}$$

On seeking the mean of these two pressures we should have

$$\frac{197 + 159}{1} = \frac{356}{2} = 178 \text{ mill.}$$

We shall be enabled to arrive at the expression of this mean by the following arrangement:

$$\underbrace{\overset{\text{The highest point.}}{105 + 105} \qquad \overset{\text{The lowest point.}}{85 + 85}}_{\text{Sum, 380 millimetres.}} \qquad \underbrace{\overset{\text{1st mixture of blood \& subcarb. of soda.}}{25 + 105} \qquad \overset{\text{2d mixture of blood \& subcarb. of soda.}}{25 + 85}}_{\text{Sum, 240 millimetres.}}$$

$$\text{Mean pressure, } \frac{380 - \frac{240}{10}}{2} = \frac{380 - 24}{2} = \frac{356}{2} = 178$$

The principle which he deduces from these experiments is thus stated by him: "From the identity of these results with the preceding, we may irrevocably conclude, that the force with which a molecule of blood moves, be it in the carotid or aorta, &c. is in all respects equal to that which moves a molecule in the smallest arterial branch;* or, in other words, that a molecule of blood moves with the same force in the whole tract of the arterial system; which *a priori* we with all other physiologists were far from thinking to be the case."†

The blood, in the experiment, is not placed under circumstances analogous to its condition in the animal system. It is clearly confined to the space A B G, and is wholly at rest in the interval of the contractions of the heart. The arterial fluid is acknowledged to be in constant motion, and consequently the two cases are exceedingly dissimilar. The gross fallacies involved in these experiments will be best exposed by the diagram, Plate III. Fig. 4, *a*, arch of the aorta; *b*, descending aorta; *c*, trunk of the carotid artery; *d*, the left subclavian artery; *e*, *e*, the common iliac arteries; *f*, *f*, the internal iliac arteries; *g*, *g*, the external iliac arteries; *h*, *h*, the epigastric arteries.

Poiseuille, in common with all physiologists, regards the circulatory system as full, so that every successive quantity propelled by the left ventricle will produce two effects,—arterial distension and the forward motion of the column of blood. Were one instrument to be fixed in the trunk of the carotid artery, and another in one of the epigastric arteries, reasoning *a priori*, Poiseuille observes, that it would be expected that the blood of the former artery, from its nearness to the heart, would act upon the mercury with greater force than that of the latter, which is not the case, nor ought such to be the result. The instrument at C is certainly nearer the heart than the one at *h*, but as the blood in each artery, and that occupying the space A B G, in both instruments, cannot flow forward in the intervals of ventricular contraction, it must be considered as a column extending to the heart, the force of which will necessarily be exerted equally on the contents of each artery and corresponding instrument. The same impulse being impressed on each column, the instrument will inevitably furnish corresponding results. The propulsive power is unquestionably the same, and if dissimilar effects were produced, these would be referable to a difference in the length of each column acted upon, and not to the blood becoming slower as it proceeds in its course, which Poiseuille imagined might cause the instruments to indicate different degrees of pressure. This condition is annihilated by the experiment, and consequently it is vain to expect that an indication of a power which it destroys.

---

* Bichat, in his "Anatomie Générale," on the limits of the heart's action, denies this equality of forces in the trunks, branches, and ramuscles; but, notwithstanding the respect due to the opinion of so great a physiologist, we find ourselves under the necessity of entertaining the opposite view.

† When we say that this force is the same in the whole tract of the arterial system, we do not mean to reject the modifications which it must experience in certain points which present a special arrangement, as the anastomotic arches of the arteries of the mesentery, the arterial circle of Willis, &c.

These remarks are sufficient to show that the conditions of the circulation are greatly disturbed by the application of the instrument, and hence the effects observed do not warrant the conclusions drawn from them. The instrument is perhaps better calculated to determine the amount of the propulsive power of the heart than any that has yet been devised; but its fitness for this purpose renders it altogether inapplicable as a measure of the force by which the blood is moved in different portions of the arterial system. The two questions are very distinct. The one is comparatively simple, the other is exceedingly complex. The blood in the artery and that in the space A B G, which forms a part of this column, is not in motion in the interval of the contractions of the heart, and cannot even, when acted upon by them, move forward beyond a few lines, and hence the pressure exerted on the mercury is clearly a measure of the contractile power of the left ventricle. The force with which the blood circulates in different parts of the arterial system cannot be determined by any instrument which arrests its motion. The equal momentum of the blood throughout the body is regarded by Poiseuille, not as produced by the heart, but by the elasticity of the arteries, which comes into play immediately after the contraction of the ventricle. This secondary power, however, can operate only when the fluid on which the arteries act is enabled to flow forward, which is impossible in these experiments. The mercury ascends in the systole of the heart, but in the succeeding diastole, when elasticity is supposed to be exerted, it does not ascend still higher, but falls, which would not be the case, according to the views of Poiseuille, were its condition analogous to that of the blood in the arterial system. This physiologist has endeavoured to prove, as already stated, that the elasticity of the arteries is even greater than the force by which it is excited. If this be the case, how comes it to happen that the mercury ascends only at times corresponding with the contraction of the left ventricle, and invariably falls with its succeeding dilatation? If the two forces were equal and consecutively brought into play, this effect would not be observed. The height to which the mercury ascends by the contraction of the ventricle ought at least to be maintained if not exceeded by the elasticity of the arteries, according to the doctrine here insisted upon. If the arteries exerted the power which he states, this instrument ought to afford some indication of it, but none whatever is perceived. The manner in which this equal momentum is imagined to be maintained throughout the body is thus explained by him.

" When the heart contracts, a wave of blood is forced into the arterial system already full of blood; the phenomena which immediately follow the projection of this wave are, the dilatation of the arteries, a sort of locomotion of the whole arterial system, by which the curvatures tend to even themselves. These phenomena cannot take place but at the expense of the force with which the wave is ejected by the heart. But scarcely has the arterial system dilated, scarcely have the curvatures yielded to the action of the heart, when, by virtue of the elasticity of the arterial system, all the arteries recoil on themselves, restore to the force of the blood all that it had just lost, and in consequence, the force with which the wave of blood is ejected from the heart retains the same intensity to the last arterial ramifications, as our experiments have proved."*

We cannot appeal to experiments in evidence of the truth of this doctrine, for these annihilate the very powers they are proposed to measure. To arrest the motion of blood in an artery, in order to determine the force with which it moves in its ordinary conditions, is a new mode of philosophizing. It would be quite as correct to interrupt the course of a stream until it overflowed its banks, to ascertain the strength of the current when unimpeded; yet such is the method of investigation adopted by ingenious men familiar with physical science. Hales, in his experiments on the motion of blood in the arteries, observed that the blood rose and fell in the tube connected with them at times corresponding with the contractions and dilatations of the left ventricle. The blood in the tube, and the mercury in the instrument of Poiseuille, have precisely the same relations to the heart. Each becomes a continuous column extending to this organ, and is urged forward in the same manner by its direct impulse. In neither case can the elasticity of the arteries be brought into play. If the doctrine of Poiseuille were well-founded, would not the blood issuing from arteries in different situations of the body be projected with equal force, every particle possessing the same intensity of motion? This, however, is not the case, "as the distance from the centre increases, the circulation," remarks Richerand, "slackens from several causes, and the blood could not reach all the parts of the body, if the arteries, whose vitality increases with their distance from the heart, and as they become smaller, did not propel it to all the organs. A very remarkable difference is observable between the blood which is sent to the toes, and that which goes to the *mammæ*, as I have several times noticed in removing the carious bones of the toes, or in extirpating cancerous breasts. The small arteries of these parts are nearly of the same size, but the jet of blood is sent to a much greater distance when one of the mammary arteries is divided." * The blood in these arteries is not at all impeded in its course, as in the experiments of Poiseuille, and consequently the character of the stream emitted may be regarded as indicating the difference in the force with which it is propelled. Poiseuille contends that this difference is attributable to the influence of respiration. In his experiments, however, neither the respiration nor the circulation is natural, and therefore it is unphilosophical to compare vital phenomena involved at this time, with the undisturbed operations of the system. The breathing, from the constrained position and suffering of the animal, becomes frequent and violent, and the arterial system generally distended, hence these important functions have relations to each other very unlike what ordinarily exist. The stream emitted during surgical operations conveys a much juster idea of the force with which it is moved, than can be derived from the elaborate experiments of this physiologist. Its escape in the one case is not at all interrupted, and consequently it is strong or feeble, according as it is near to or remote from the heart. In the other, as already remarked, it is confined to a tube, and its tendency to flow forward is arrested by a column of mercury. The important changes produced by tedious and painful experiments on the vital functions are not only frequently overlooked, but are extremely difficult of calculation. The truth of this is forcibly exemplified by the experiments of Poiseuille. He endeavours to prove that the blood is circulating with equal force throughout the arterial system, from the contraction of the left ventricle and the elasticity of the arteries. The latter cause is regarded as exerting the same degree of influence as the first, and as their action is alternate, the blood is necessarily in constant motion. According to this view, the blood injected into the aorta by the contraction of the left ventricle acts upon a column already moving rapidly forward, and not upon a column at rest. The same impulse impressed on two equal columns, the one in motion, the other at rest, would produce very different results. It is comparatively easy to estimate the force necessary to move the one, but it is impossible to calculate the powers employed in moving the other. An appeal to physical science will be vain, for there is nothing strictly analogous in the whole range of mechanical operations. The blood in the arteries circulates principally from the combined influence of respiration, muscular contractions, the action of the capillaries, the impulse of the left ventricle, and that of previouscontractions, and to a slight extent the elasticity of the vessels. The position in which the animal was placed in the experiments of Poiseuille, conjoined with its sufferings, would greatly disturb the ordinary conditions of the arterial fluid, and consequcutly affect the accuracy of the results obtained. All obstacles to its regular and uninterrupted flow will unquestionably destroy the influence of three of these powers, the agency of the capillaries, elasticity, and the impulse of previous contractions, and hence, the column in the experiments of Poiseuille is acted upon chiefly by the heart only. If two instruments, therefore, be applied to arteries differently situated in respect of this organ, they will indicate the same pressure, but clearly from the annihilation of these powers which produce important modifications in the character of the circulation. This temporary and unnatural condition of the arteries, is what Bichat imagined to exist at all times. He looked upon the arteries as exerting no influence whatever on the motion of blood, nor did he believe in the causes usually assigned as retarding it, such as its passage into vessels, the conjoint area of which increases as they subdivide, friction, angles and anastomoses. His experiments are liable to the same objections as those of Poiseuille. They prevent the operation of the powers which it is his object to analyse. The following experiments were imagined to be peculiarly favourable to his views.

---

* Journal de Physiologie, Tome viii. pp. 296, 297.

---

* Elements of Physiology.

" The force of the heart causes the blood to circulate in inorganic tubes fitted to arteries for a considerable distance. If an inch of the carotid artery be cut away, and a tube be substituted, fixed in the two open ends of this artery, the blood will traverse this tube, and cause it to pulsate as in the ordinary case. I cannot conceive by what circumstances those have been deceived who have obtained different results. 7. Fit a tube to an artery, and to the other extremity of the tube let a pouch of any membrane, or of gummed silk be attached, and the blood will fill it immediately, and, further, at each contraction of the heart, it will exhibit a sort of pulsation. It is thus that the aneurismal tumour, though cellular, pulsates. Whatever be the organ, which contributes to form the cyst, it will equally pulsate, provided it receive through the blood the impulse of the heart."

In the first experiment the blood continued to circulate, and pulsations were felt beyond the insertion of the tube, and hence he inferred that the heart was the only propulsive agent. In the second the pouch was quickly filled with blood, and exhibited pulsations synchronous with the contractions of the left ventricle; an effect which might have been anticipated. It is manifest that in both cases, but especially in the latter, all motific powers except that of the heart are destroyed, and consequently no positive deduction can be drawn from them. There is, indeed, the same objection to all the experiments of Bichat instituted to determine the causes which move the blood. The removal of a portion of the carotid artery and the substitution of a tube, in the manner which he describes, is an operation both tedious and painful. It cannot possibly be performed without producing great disturbance in the ordinary conditions of the circulation. The arteries, as already remarked, invariably become distended under such circumstances, so that a series of inert tubes would carry on the circulation as well as organic vessels.

The second experiment presents a still wider departure from these conditions. In this the blood is confined to the artery and tube, and has no means of escape or progression beyond the pouch, so that every contraction of the left ventricle must necessarily be communicated to the latter, with the same force and regularity as if the artery were an inorganic tube. Had the objections to these experiments been fully understood, succeeding physiologists would have avoided falling into errors scarcely less serious than those which have been exposed.

We now proceed to the investigation of another important subject, the condition of the arterial system immediately preceding the contraction of the left ventricle. All authorities regard it as full of blood; but there is great difference of opinion concerning the manner in which it is influenced by the injection of every fresh quantity. The whole mass of blood is considered to be pushed forward, and the arterial system at the same time dilated. The force which is thus expended is imagined to be restored by the recoil of the arteries, so that the power of the heart, according to this doctrine, is transmitted undiminished to every portion of the system. Against this reasoning it is argued, that the small quantities of blood ejected by the left ventricle are insufficient to produce these effects, and, moreover, it is stated, that were such the case, a greater quantity would be discharged by the arteries than is returned to the ventricles. To obviate this difficulty the supporters of the doctrine contend, that the contractions of the arterial system are not synchronous, but rapidly successive in the intervals of the impulse of the heart. The portion nearest this organ is imagined to be first dilated, which immediately contracting distends the next, and thus a third, causing in quick succession a series of dilatations and contractions, the effect of which is to force from the extreme arteries as much blood as is sent into the aorta by any single contraction of the left ventricle. The recoil of one portion is supposed to occur when the next in advance is in a state of relaxation, and that behind is approaching the state of greatest contraction, so that the blood is easily pushed forward, and cannot possibly retrograde. Neither view is founded on ascertained facts. Indeed it is not possible to prove either the one or the other. The opinion, that the recoil of the arteries urges the blood along its course, with a force equal to that of the heart, is an assumption, nor can any means be devised, by which this secondary power can be accurately known or measured. Neither the experiments of Poiseuille, nor the manner in which blood flows from divided arteries, can throw any light on the inquiry. The latter fact is alluded to by physiologists, as furnishing the most satisfactory evidence of the influence of elasticity on the arterial current. Magendie remarks:

"It is, however, very easy to prove that it is not in this way that the blood moves in the vessels. When a large artery is opened in a living animal, the blood escapes, forming a continuous jet, but with a violent jerking. The same phenomena take place in man, when the arteries are opened, whether by accident or in the course of a surgical operation: the heart not being able to occasion a continuous efflux, its own action being intermittent, it must be that the arteries act on the blood; and this action can be no other than the tendency which they have to close on themselves, and even to obliterate their own bore altogether."*

He states that, were the heart the only propulsive power in operation, the blood would not flow from divided arteries in a continuous stream, marked simply by jerks, corresponding with the contractions of the left ventricle. It would be ejected only contemporaneously with these contractions. We shall attempt to show that the modification in the character of the stream emitted is not explicable on the action of elasticity, and, therefore, the phenomenon cannot, as it is contended, be adduced as confirmatory of the doctrine. The arterial fluid after the contraction of the left ventricle, independently of the influence of elasticity, is greatly agitated, and has a tendency to rush forward, but its progressive motion is necessarily very limited, from the obstacles opposed to its course. Suppose, then, the whole of these obstacles removed, how would the fluid act? It would not be at rest in the divided artery, but would be urged forward by the rush of blood in this direction from other parts of the arterial system. Respiration and muscular contractions are constantly modifying the current of blood; in one situation urging it forward, in another arresting its progress, but in all cases exerting pressure, in consequence of which the blood will escape if an opportunity occur. A divided artery presents this occasion, and the manner in which the blood is ejected establishes the correctness of the explanation. A much greater quantity of blood is emitted in a given time from the divided artery, than would have flowed through it in the ordinary conditions of circulation.

---

* Magendie, Journal de Physiologie, Tome i. p. 108.

From this fact we may draw two important conclusions; first, the tendency of the arterial fluid to flow in this direction, and secondly, that the whole length of the artery and its associated branches are exceedingly distended. The exercise of elasticity in these vessels is impracticable at this time. To contend for its operation is in the highest degree unphilosophical. It is imagined to act in the undisturbed conditions of the arterial system, from the consideration of one circumstance, which cannot exist in cases of divided arteries. The arterial system is imagined at all times to be full of blood, so that the injection of an additional quantity causes distension, of which the arteries relieve themselves by their recoil during the dilatation of the left ventricle. The recoil takes place, because the heart does not transmit a continuous stream. In the case of divided arteries, a continuous current is produced, and consequently the action of elasticity is prevented. How can an artery act upon its contents when it is kept in a state of constant distension?

On the subject of elasticity, Magendie remarks, "The elasticity of the arterial parietes represents that of the reservoir of air in certain pumps having an alternate play, and which, nevertheless, givê out the liquid in a continuous stream; and in general we know that in mechanics, every intermitting movement may be converted into a continuous movement, by employing the force which produces it, so as to compress a spring which reacts continuously."*

Physiologists generally adduce the same kind of evidence in illustration of the influence of the arteries on the motion of blood. It is, however, anything but satisfactory. A phenomenon occurring in an unnatural state of the vital powers can seldom if ever afford a correct idea of their action in an undisturbed condition, and certainly the case in question forms no exception. The way in which blood flows from a divided artery throws no light on its motion in the same class of vessels in the ordinary process of circulation. Water in the pipes of a fire-engine is very differently circumstanced from blood in an artery, and the continuous stream which escapes from both is not produced by the operation of similar causes. The prevailing opinion is, that the causes are precisely the same in character, and hence the supposed aptness of the illustration. The water, previous to receiving the stroke of the piston, is at rest, nor would it exhibit any tendency to escape, were the pipes to be punctured in the least dependent situation.

---

* Magendie, Journal de Physiologie, Tome i. p. 110.

The reaction of the reservoir of air after the stroke of the piston may be regarded as a second impulse as purely local as the first, and acting in the same manner on the column before it. In the animal machine, the apparatus securing the circulation is not only much more complicated than the one described, but produces correspondingly more complicated effects. The blood-vessels are of three kinds; the arteries, the capillaries, and the veins; and are always in a state of distension. The rate of circulation differs materially in each class of vessels. The causes keeping the blood in constant motion are various. Respiration and all the muscular contractions of the body urge it forward or modify its course. The capillaries, by drawing the blood from the arteries, give it a marked tendency to move forward, and by pouring their contents into the veins, necessarily agitate the current flowing towards the heart. It must also be taken into account, that, independently of the operation of these causes, the previous, not the immediate impulses of the left ventricle, exert an important influence on the motion of the blood. Were the arteries open like the pipes, the force of each ventricular contraction would be expended on the fluid ejected, but as the fluid acted upon still remains to be again agitated or pushed forward, it is quite evident that the whole mass is in motion, and would readily escape, without the direct agency of the heart, were any part of the circulating system, but especially the arteries punctured. An opening in any portion would cause the blood at once to rush out with impetuous force. This would occur were the impulse of the heart interrupted, and not from the elasticity of the vessels, the influence of which is imagined to be aptly illustrated by the reaction of the reservoir of air on the column of water. The tension of the arterial system is not occasioned by elasticity. It is an effect which the exercise of elasticity relieves. The blood thus confined escapes with violence from the divided or punctured artery, not purely in consequence of the direct action of the heart, nor of the elastic or contractile properties of the vessel, which is the doctrine taught, but from the little resistance which the atmosphere offers to the escape of its pent up and active molecules. The blood which flows first is always propelled to a greater distance than that which subsequently escapes, and the explanation here given accounts for the phenomenon. The tension of the arterial system is at once relieved by the first portion of the fluid ejected. The continued force with which it flows arises from the action of the heart, and the removal of all resistance in the direction of the external opening. The tendency of the blood to rush in this direction will necessarily convert an intermittent into a continued current, distinguished by jerks corresponding with the contractions of the left ventricle. The water in inorganic pipes is not thus circumstanced. It is acted upon by one cause only, at any one moment, either the piston or the reaction of the reservoir of air, and it has no tendency to escape, except from the successive action of these causes. Hence it is manifest there is the greatest possible difference in the condition of the two fluids, from the striking difference in the complexity of the causes in operation.

The manner in which blood flows from divided arteries has been regarded as affording evidence of the laws by which it is regulated in the undisturbed states of the circulation. Magendie indeed draws this conclusion from the fact, " that in the arteries the blood is not alternately in repose and motion; but that it is moved in a continuous manner with occasional jerking in the trunks and branches, and in a manner uniformly continuous in the ramuscles and their extreme divisions."*

The fact does not authorize this inference, nor does it throw any light whatever on the functions of the circulating system, nor is there any phenomenon which elucidates the inquiry. Whether the blood in the interval of the contraction of the left ventricle be comparatively at rest, or urged forward with a force equal to that of the ventricle, is a question requiring elucidation. Experiments have not lessened or dissipated the uncertainty, and it is purely hypothetical to assert that the elasticity of the arteries maintains the current in a uniformly progressive movement. Experiments on divided arteries, if they prove anything, show that such is not the case. The blood even under these circumstances is accelerated during the systole of the left ventricle, and is projected with less force during its diastole. According to the argument, that the two forces are equal, such difference ought not to occur. It is observed, however, when there are many concurring causes facilitating the flow of the blood during the interval of the ventricular contraction, and it is certainly reasonable to infer, that the difference will be much greater, when the principal of these causes, the removal of all the obstacles to the motion of the fluid, no longer exists.

It will perhaps be admitted from these strictures, that the exercise of elasticity is not proved by the character of the stream ejected from divided arteries. It is not, however, intended in these pages to deny entirely the influence of this property on the motion of blood. Its existence was as fully appreciated by the earlier as by modern physiologists, nor have the elaborate researches of the latter added anything of importance to our previous knowledge of this interesting but abstruse question. The arterial fluid is admitted to become slower as it proceeds in its course; and can it be doubted that the circumstances which cause this effect diminish the pressure or force with which it circulates? Friction, and the blood passing into vessels, the conjoint area of which augments at every step, will enfeeble its motion. The fact of its flowing from divided arteries of the toes with much less violence than from those of the mammæ, throws perhaps more light on this difficult inquiry than any other phenomenon with which we are acquainted. The account which is given of capillary circulation by Poiseuille and Magendie, does not strengthen the opinion that the pressure of the heart is transmitted directly to the venous system. It requires some effort to conceive the globules as carrying forward the momentum impressed upon them by the left ventricle, and as being in fact parts of one uninterrupted chain extending from the left to the right side of the heart, without the aid of which circulation would be arrested in the veins. This is the doctrine taught, and yet these globules, so important as links of continuous motion, are described as sometimes being almost at rest,—carried in a retrograde direction, or as traversing the capillaries alone, or two or three abreast, often with very little progression. Were the arterial and venous system connected by obvious currents of blood, the influence of the one might be imagined to be transmitted to the other, but when the connection is considered to be maintained only by globules, frequently pursuing their course alone, or moving in a devious direction, the doctrine, that large masses of venous blood are propelled by them,'is not without serious difficulties. In what manner are they affected? Do the small streamlets first formed by the capillaries act upon the columns before them, and these upon others still larger, until at length the contents of the *venæ cavæ* receive the impulse of the heart. Globules circulating in the way described cannot possibly influence masses of blood.

---

* Journal de Physiologie, Tom. i. p. 115.

Leaving this subject for the present, we pass to make a few further remarks on the condition of the arterial system in the systole and diastole of the left ventricle.

The quantity of blood ejected into the aorta at every contraction of this cavity is computed at about an ounce and a-half. The energy with which it is propelled is expended against the arch of the aorta, producing a vibratory motion, and upon the mass of blood before it. The arterial distension is supposed to be relieved by the recoil of the arteries during the diastole of the ventricle. The difference in the motion of arterial fluid or of the forces employed in the systole and diastole of the left side of the heart, is a problem which physiologists cannot possibly solve. The contraction of the ventricle and the subsequent recoil of the arteries urge forward a quantity exactly proportionate to what is received in the same time by the right auricle. Were the former alone to displace a quantity equal to what is sent in, the additional action of the arteries would disturb the relation between the two cavities, by transmitting to the venous more blood than could possibly be removed by the right side of the heart. The quantity ejected by the left ventricle displaces a quantity less than that sent in, by as much as is required to distend the arterial system. The difference in the rate at which blood moves in the arteries during the systole and diastole of the heart is perhaps a question which will ever remain undetermined. Patient study of the phenomena of circulation certainly leads to the conclusion, that the arterial fluid is urged forward slowly and feebly in the diastole of the heart. The condition of the aorta after the contraction of the left ventricle does not appear favourable for the exercise of elasticity. The blood ejected, in acting on the column before it, will leave a space immediately at the roots of this vessel only partially occupied, so that in the diastole of the ventricle a small quantity of blood will have a retrograde motion, and, according as this relieves the arterial distension, the agency of elasticity will be proportionately diminished.

The supposition which is here advanced respecting the condition of the arch of the aorta during the diastole of the left ventricle, receives, from the researches and reasoning of others, confirmation previously unknown to me. The fact is not, however, employed to show its effects on the exercise of the elastic properties of the arteries. If it exist it will be impossible to deny that the tension of the whole of the arterial system will be modified by it. The opinion is thus supported by Burdach.

"If the wave of blood is projected to a certain distance in the artery by the effect of the systole, but again flows back (714,6°) towards the sigmoid valves, which are meanwhile closed on themselves, (708,3′) this phenomenon equally supposes a space not entirely occupied by blood, and as, during the diastole of the heart, the blood continues to flow from the arterial trunk into the branches; (714,11;) but this trunk is not so flexible as to be able to contract itself in the same proportion, we are compelled to admit equally in this case, either a vacuum, or a space containing air. Fontana sets in opposition to this opinion an experiment made by himself, in which the aorta, when he punctured it near the sigmoid valves, emitted blood during the diastole of the heart, only in somewhat smaller quantity than during the systole. But he had no reason to draw a conclusion from this hostile to our views: for we admit only that a certain extent of the aorta is not completely filled; we do not contend that it is absolutely empty; and even in this latter case the blood would still issue through the wound. Spallanzani has observed that, in salamanders, the bulb of the aorta contained but little blood during the diastole of the ventricles, but that did not close at all when the circulation was feeble, so that then it had a pale colour, and when opened, allowed no fluid to escape."—Vol. vi. p. 288.

Modifications are continually occurring in the distribution of the blood, which require corresponding changes in the capacity of the arteries. At one time these are suddenly and greatly distended, at another the powers of life are so exhausted that the pulse is scarcely felt. How immense is the difference in the quantity of blood circulating at these times. The arterial system is not only liable to such general changes in connection with disease, but partial and momentary modifications occurring during the action and healthy exercise of the body. In one part, circulation is impeded, in another it is accelerated, the blood being urged forward with a strong and irregular force. These varying conditions of the circulation demand corresponding modifications in the arteries, which can be secured only by their elastic and contractile properties. The experiments of Magendie and Poiseuille have been shown in the preceding pages to be defective, not warranting the conclusions deduced from them. The former physiologist adverts to the practical importance of the views resulting from the supposed fact, that the blood is moving with an equal momentum throughout the system. On this point he thus forcibly expresses himself:

"This equality of pressure throughout the whole of the arterial system is a fact highly important in regard to practice. If you wish to diminish the mass of fluids, it is of little importance whether you open this or that artery; for the equilibrium is simultaneously restored in the vascular canals, and the parietes of each tube feel themselves relieved of a pressure everywhere uniform. It is physicians who attach a grave importance to the choice of an artery to be subjected to the lancet. Thus the temporal artery is the one generally preferred in cerebral affections. But these are mere scholastic dogmata, having no basis themselves, and being repugnant to sound theory. The truth in this, as in a great many other cases, is more simple than the imagination would represent it. Where then the use of loading the memory with so many erroneous precepts recorded in so many books regarding the vessel proper to bleed from in this or that disease? The single fact of the equality of pressure wonderfully simplifies this question."*

The experiments in favour of the equality of arterial pressure have been shown to be liable to serious objections, and, consequently, the application of this principle to medical science may be fraught with much mischief. Experience has repeatedly proved the value of the discrimination which he ridicules, and it would be highly injudicious, on many occasions, not to make the selection of the artery a matter of importance. Local bleeding certainly often allays general arterial excitement; but this does not establish the doctrine here laid down. The relief may arise from the treatment subduing local irritation, the cause of the constitutional symptoms; or the action of the heart may be quieted by the loss of blood, or, indeed, much of the good attributed to the practice may be owing to the conjoint employment of means, whose beneficial agency is entirely overlooked in the calculation.

---

* Leçons sur les Phenomenes Physiques.

In the further prosecution of this inquiry, several other important experiments of Magendie and Poiseuille, bearing especially on the tension of blood in the arteries and veins, will be examined, and it will then be shown that the experiments are in their nature defective, and incapable of furnishing accurate results. We must certainly have much better evidence than that with which we are favoured, before we are disposed to adopt the novel doctrine respecting the equality of tension throughout the arterial system, or the startling conclusion, that the selection of the artery is quite a matter of indifference, when imperative to have recourse to the abstraction of blood. Before yielding assent to a doctrine of such practical importance, the evidence on which it rests must be free from all doubt and speculation. The experience of ages is opposed to it; the results of daily practice give it no colouring or support; and it is at variance with the views and reasoning of all antecedent physiologists; and it will be shown in subsequent investigations that it is altogether founded in error.

After the preceding investigations, the pulse naturally comes under consideration. There is now little difference of opinion respecting its cause. The view which Parry entertained is generally admitted, and its accuracy appears to be placed beyond all question by elaborate and carefully conducted experiments. There are points, however, connected with this subject on which physiologists are by no means agreed. It is contended by one class of writers, that, though the pulse is produced by the contraction of the left ventricle, the contractions of the arteries in different parts of the body are not synchronous with that of the ventricle, but rapidly successive. The earlier physiologists, Harvey and Haller, assert that the contractions of the arteries are synchronous with the systole of the heart, and my own researches are decidedly in favour of this conclusion. The following extract gives some of the authorities who think otherwise, and the reasons for the doctrine which they maintain :—

"Weitbrecht, Liscovius, and E. H. Weber* have shown, however, that this is not the case. The pulsation of the arteries near the heart is synchronous with the contraction of the ventricle. But at a greater distance from the heart, the arterial pulse ceases to be perfectly synchronous with the heart's impulse, the interval varying, according to Weber, from one-sixth to one-seventh of a second. Thus, the pulse of the radial artery even is somewhat later than that of the common carotid. The pulse of the facial, at about the same distance from the heart, is isochronous with the pulse in the axillary artery; while the pulse is felt somewhat later in the metatarsal artery on the dorsum of the foot, than in the facial artery and common carotid. Weber[*] has explained the cause of this difference. If the blood circulated in perfectly solid tubes, whose walls admitted of no extension, the impulse of the blood, driven by the ventricle into the arteries, would be communicated even to the end of the column of blood, with the same rapidity with which sound is propagated through this fluid,—much quicker, namely, than in atmospheric air; the pressure of the blood would be transmitted to the finest extremities of the arteries, with no perceptible loss of time. But, in consequence of the arteries admitting of some extension, particularly in length, the impulse given to the blood by the heart distends first merely the arteries nearest to the heart. These, by their elasticity, again contract, and thus cause the distension of the next portion of the arterial system, which also, in its turn, by contracting, forces the blood into the next portions, and so on; so that a certain interval of time, although a very short one, elapses before this undulation, resulting from the successive compression of the blood, and the dilatation and contraction of the arteries, reaches the most distant branches of the arterial system."[†]

---

* In the Treatise, De Pulsu non in omnibus Arteriis plane synchronico.

The same view is also adopted by Carson in his Inquiry into the motion of the blood, and is supported with his usual ingenuity and aptness of illustration.

In confirmation of his opinion, Haller remarks, "Sed etiam in homine, si manum dextram cordis sedi opposueris manum sinistram arteriæ temporali, labiali, radiali, popliteæ adplicueris, manifesto percipies, eodem omnino tempore et cordis recurvatum apicem costas ferire, et sanguinem in omnibus arteriis, quas nominavi, pulsum efficere. Experimentum sæpe feci, et in me, et in vivis animalibus; fecit Harveius;[‡] fecerunt primi circuitus sanguinis statores;[§] fecerunt nuperi Cl. viri;[||] fecit in equo Cl. Bourgelat."[¶]

This experiment of Haller[**] we have repeatedly performed with the greatest possible care, and under circumstances favourable for the attainment of truth, and always with the same result. Our own experimental researches are not alone the cause of objection to the doctrine which is here combated. The consideration of the evidence by which it is supported furnishes the best reason. It rests indeed upon pure assumption. It is stated that "the impulse given to the blood by the heart distends first merely the arteries nearest to the heart." The doctrine is founded altogether on this supposed fact. From its importance it would be imagined that clear and unquestionable evidence would be adduced in its confirmation. Argument only is brought forward, and certainly not of the most convincing character. Whether the impulse of the heart be directed against a column of blood in arteries possessing elasticity, or altogether destitute of it, there is one effect that must take place,—the forward movement of the fluid acted upon. Between arteries and inorganic tubes, there is a difference which would simply modify the effect. In the former, the blood ejected by the ventricle would not remove an equal quantity from the extreme arteries, but only the surplus of that which distends the arterial system. In the latter, an equal quantity of fluid would necessarily be discharged. The pulse unquestionably depends on this forward rush of the blood, and it follows as an inevitable consequence, that the impulse must be transmitted instantaneously to all parts of the arterial system. To state that the blood propelled distends only the arteries nearest to the heart, is an assertion resting on no facts nor founded on any probable data. It is equivalent to saying, that it foregoes acting on the column before it. If it acts upon it, which it must inevitably, the impulse of the ventricle will be felt at the same instant of time throughout the body.

---

[*] Annotat. Anatom.

[†] Müller's Elements of Physiology, p. 200, 201.

[‡] Exercit. i. c. 3.

[§] J. Walæus, Epist. cit. p. 406, ed. 1.

[||] Josephus Duverney de l'ouie, p. 207, de pulsu in aure locutus. Thomas Schwenke, l. c. p. 82.

[¶] Hippiatrique, Tome ii. p. 346.

[**] Elementa Physiologiæ, Lib. iv. Sect. xli.

In opposition to the view which is here advocated, Dr Carson remarks, "upon the supposition of a synchronous contraction of all parts of the arterial system, the quantity of blood discharged in consequence of that contraction, from the termination of the arteries, must greatly exceed that which would be returned to the ventricles, even if there were no valves to oppose; the quantity which would be discharged from the former, would be to that returned to the latter, as the square of the diameter of all the capillary arteries of the aortic system, to that of the diameter of the aorta alone."

With reference to the opinion of this physiologist, it may be asserted, that the simultaneous contraction of the arteries will not produce the result which he regards as inevitable,—in fact, it may be shown to be an impossibility. All authorities admit the arteries to be full previous to the injection of a fresh quantity into the aorta, and, however greatly they may differ on the influence which they exercise in consequence of elasticity or contractility, on one point there can be no discrepancy of sentiment, viz. that the arteries will be dilated, though not to any perceptible extent, by every addition to the moving column. The arteries are not rigid unyielding tubes, and consequently a dilatation to some extent must take place. The fact cannot be disputed. Accordingly, the quantity of blood ejected by the left ventricle produces two effects,—the forward rush of the column and the dilatation of the arterial system. It is therefore an impossibility that, on the contraction of the left ventricle, the discharge of blood from the extreme arteries, can exceed that which would be returned to the ventricles in the same time. The simultaneous contractions of the arteries, and the time intervening between the next contraction of the left ventricle, conjointly furnish the quantity precisely equivalent to that which is transmitted in the same time, to the cavities of the right side of the heart. There are two periods to be considered, the one occupied by the contractions of the arteries, and the next by that of their subsidence. The reasoning of Carson leaves altogether out of consideration the second period. The effect he contends for could take place only on the supposition, that the arteries are rigid non-elastic tubes, which few will be disposed to assert.

The view which is here given explains satisfactorily the way in which the balance of circulation is maintained between the arteries and veins, without the necessity, which is implied by the doctrine of Weitbrecht, Liscovius, Weber, and Carson, for the successive contractions of the arteries; which the last physiologist states "cannot be discerned by the senses." It cannot be very bold to question what does not admit of proof.

In connection with the consideration of the functions of the arteries, their usually empty state after death is a phenomenon which solicits examination. The views proposed to explain it are various. By one it is contended that the elastic property of the arteries at the moment of death forces onwards the blood contained in them. By another, it is ascribed to the tonic contraction of the arteries; by a third, the resilience of the lungs and of the coats of the arteries is adduced to account for the accumulation of blood in the veins. It is argued by a fourth that the effect arises from the attraction of the capillaries. It is not our intention to enter at this time into a lengthened inquiry concerning the phenomenon, as it will again be noticed when the functions of the capillaries are studied, regarding these vessels as instrumental in producing the result. All writers on this subject appear to have passed over in silence the gradual changes which are taking place for hours, in the generality of cases, previous to death. This inquiry is exceedingly important, and will remove much of the apparent difficulty investing the phenomenon.

During the period of dying, the action of the heart gradually becomes feeble and mostly frequent, until at length it ceases to be detected in the slender streamlet flowing along the arteries, though it may still be discovered on applying the ear to the chest. Whether the period be long or short, the contents of the venous system are gradually acquiring a preponderance over the arterial, not, indeed, from any of the causes enumerated by physiologists, but from the exercise of the ordinary powers of circulation. At the moment of death, under such circumstances, the arterial system contains scarcely any blood at all. Thus far we require not the aid of any of these causes to account for the phenomenon. The breathing at this time is often most laborious and frequent;

a condition which would appear unfavourable, according to the hypothesis of Dr Carson, for the accumulation of blood in the veins, being widely remote in character from the subsidence of the lungs, which he regards as principally instrumental in the production of the effect. Almost the whole of the effect precedes actual death. When this is sudden, the arteries almost invariably contain a greater proportion of blood than when it is slowly produced. Were the causes assigned by physiologists efficient and active, there ought to be no difference in the results. Why should not the resilience of the lungs, the elasticity or tonic contractility of the arteries, operate equally in all cases? If any difference existed it ought to be in favour of their greater activity when death is sudden, occasioned by hanging, drowning, or suffocation from the inhalation of irrespirable gases or mephitic vapours; for under such circumstances the natural properties of the lungs and of the arteries have not been at all wasted by participating in any previous disease. Long and serious organic or functional derangement always enfeebles and exhausts the vital powers; the inherent properties of the animal system; and consequently, when they have been the least injured, those which continue in action for a short time after death ought to exhibit greater activity and energy than in cases of previous decay and exhaustion. There is not only a difference in the nature of the death, which will in part explain the discrepancy of the results, but there is also a marked difference in the proportion of the blood to the containing vessels previous to this events, to which no writer has adverted, and yet a knowledge of it is necessary to afford a correct view of the phenomena. When death is occasioned by disease which has continued for weeks or months, the circulating fluid is greatly diminished in quantity in many instances, at least, to the extent of one-half. There is no permanent decrease in the capacity of the veins corresponding to this change. It may therefore be stated, that there is a lessened quantity of blood and an unaltered capacity of the veins which may be designated the receiving vessels. In cases of sudden death there is no diminution in the quantity of the circulating fluid, and consequently it would scarcely be expected, whatever causes may be supposed to be in operation, that there would be the same facility for the escape of blood from the arteries into the veins, as in the case previously considered. Both classes of vessels are full, that is to the ordinary distension of health, which is very different from their condition after the long continuance of disease.

We may thus briefly recapitulate the striking differences in the state of the circulating system in the cases which have been examined.

1. The arterial system after death resulting from disease is

usually found empty, and almost the whole of this effect is produced immediately preceding the extinction of life.

2. The very slender streamlet which may remain is certainly too small in quantity to be urged onwards by the elasticity or tonic contractility of the arteries. They are not found after death in a cord-like condition, and nothing less than a contraction which would produce an effect of this kind would act on the lingering thread-like current.

3. The arterial and venous systems after death, produced suddenly by hanging, drowning, or any cause, are seldom, if ever, found in a completely empty state. This is admitted generally by writers.

4. As these systems previous to death are full to the ordinary distension of health, it is scarcely to be expected that the venous already thus distended should be able to receive readily the whole of the contents of the arterial.

5. There is also a remarkable difference in the two cases of death, with respect to another class of vessels, viz. the capillaries, which is worthy of consideration. Their action will be admitted to continue after death; but when this is slow and protracted, they have clearly, in common with every other part of the body, nearly ceased to exercise their functions; when, on the contrary, death is suddenly produced, these functions are unimpaired by previous disease or derangement, and consequently will long continue, the effect of which will be to draw the blood from the arteries, and transmit it into the veins. We shall in another part of this inquiry show, that the activity of the capillaries, intermediate between the two systems, will necessarily in this manner influence the contents of both. The hypothesis of Dr Carson is not required to account for the condition of the arteries after death. It is indeed perfectly gratuitous. In cases of slowly approaching death, the breathing is sometimes immediately preceding so soft and easy, that it is difficult to perceive it, and the lungs are unquestionably gradually collapsing, so that their resilience after the last expiration can make no material change in them. In cases of sudden death, the effect for which he contends ought to be the greatest, there being previously no gradual diminution of elasticity. It happens, however, unfortunately for his hypothesis, that, under such circumstances, the arterial system is seldom found empty.

ART. IV.—*An Inquiry into the Mechanical Functions of the Ear.* By JAMES SYM, M.D. Ayr.[*]

SECTION I.—*On the Mechanical Functions of the External Ear.*

THE external ear is generally supposed to be superadded to the proper organ of hearing for the purpose of augmenting the intensity of sound; but there is a difference of opinion with regard to the manner in which it accomplishes this end. The older theory is that it performs the office of an ear-trumpet in concentrating the sonorous undulations of the air upon the membrane of the tympanum; whilst some modern physiologists maintain, that its cartilages are made to vibrate by the impulse of the air, and that, in consequence of the extent of their recipient surfaces, they transmit the vibrations with greater force to the membrane of the tympanum, than if these had been only allowed to impinge upon it directly

Since, however, some animals can hear acutely, although their external ears are so formed that they cannot perform the office of ear-trumpets;[†] and since others can hear equally well, although their external ears are composed of materials which cannot be made to vibrate like sounding-boards;[‡] and since a large portion of the animal kingdom seems to hear well enough without any external ears,[§] I think we are warranted in concluding, that the principal purpose of an external ear must be something else than to augment the intensity of sound.

The organs of smell and sight are each furnished with an external apparatus similar to that of the ear. The external nose consists of an osseous passage, analogous to the *meatus externus* of the temporal bone. To the extremity of this are attached cartilaginous plates, analogous to the cartilages of the auricles. In the membrane lining its inner aspect are imbedded mucous follicles, analogous to the ceruminous glands of the ear, and producing a viscid secretion; both passages are defended by a sort of grating of hairs; and the muscles of the *alæ nasi* correspond with

---

[*] This paper was read to the Medical Section of the British Association at Glasgow, on the 21st September 1840; and in compliance with their request, I now lay it before the public.

[†] This is the case with the human ear, of which the helix and antihelix present ridges and convex surfaces, as much calculated to disperse, as the concha is to collect, the undulations of the air.

[‡] The expanded auricles of the bat, instead of being composed of rigid cartilages, capable of transmitting vibrations, consist of pliant membrane, which appears to be ill adapted for such an office.

[§] In the cetacea, reptiles, and most birds, which latter are, by no means deficient in the sense of hearing, there is scarcely a vestige of an external ear.

Fig. 1.

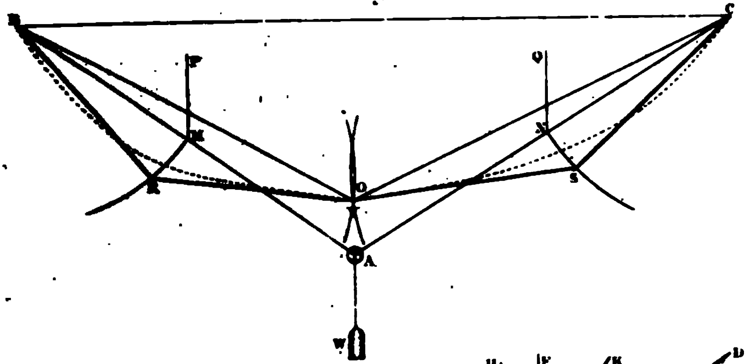


Fig. 2.

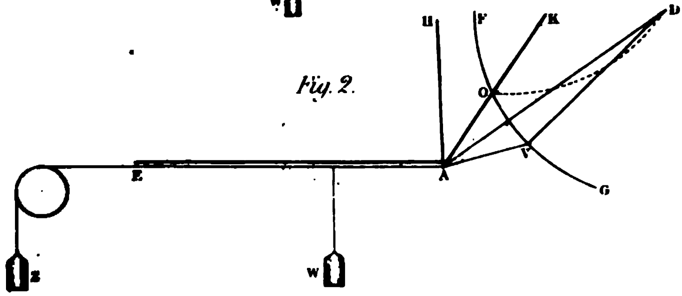


Fig. 3.

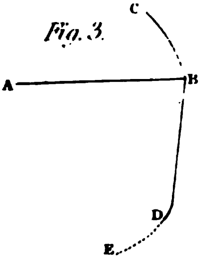


Fig. 4.

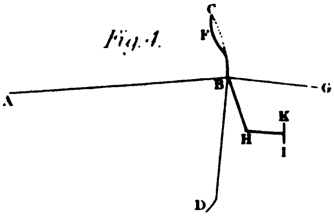


Fig. 5.

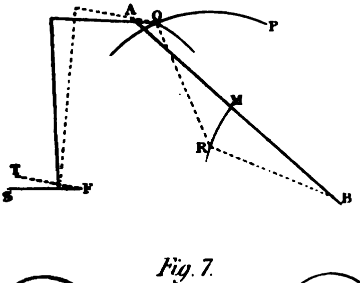


Fig. 6.

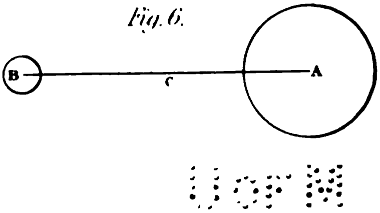


Fig. 7.

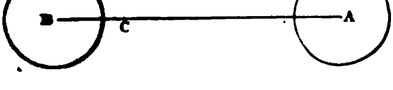
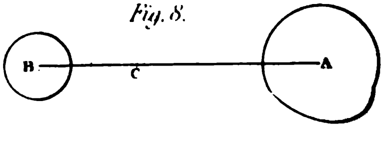


Fig. 8.



those of the cartilages of the ear. Anatomists have pointed out an equally close analogy of the external apparatus of the ear with that of the eye:*—so that the organs of smell, sight, and hearing, are all provided with appendages similar to each other in their position, their conformation, and the several structures of which they are composed; and this affords a presumption that the functions of these appendages should likewise be analogous.

Now, the essential use of the external nose, and of the eyebrows and eyelids, is to protect the organs to which they are attached, from having their structures injured, or their functions disturbed by foreign influences. The nose is a roof, under the shelter of which, the proper organ of smell performs in security its office of receiving impressions from odorous effluvia. Moisture flowing down the forehead is prevented from reaching the papillæ of the pituitary membrane; rain and snow, carried by the wind, are equally excluded; and by the hairs and viscid secretion, insects, dust, and other small bodies floating in the atmosphere, are entangled at the threshold. The eyebrows and eyelids execute precisely similar offices for the organ which is placed under their wardship; and the eyelids perform the additional duty of wiping away the residue of the tears, which must become irritating, when their saline ingredients are concentrated by evaporation.

In those of the Primates which generally carry their heads erect, the external ears are so situated, and of such a figure, as to determine the direction of the wet hair away from the orifice of the meatus, and to cause sweat and other moisture to drain off from the upper parts of the head along the channel formed between the auricle and the scalp. But these animals have so little power of moving their ears, that in order to protect the meatus from rain, snow, or flights of insects, they are obliged to turn aside their entire head. This cannot be conveniently done by quadrupeds, in which the position of the neck is horizontal, so that mere rotation of the head upon the neck is with them a very awkward movement. The auricles of quadrupeds, and of such of the Quadrumana as resemble quadrupeds in their habits, are therefore endowed with extensive mobility; and they serve to shield the meatus from foreign matters, from whatever point they may approach.†

---

* The orbital margin resembles the osseous portion of the *meatus auditorius*; the eyelids contain cartilages moved by muscles, and surrounding the entrance to the eye; and the cilia and Meibomian glands are analogous to the hairs and ceruminous glands of the ear.

† Besides defending them from storms of rain, swarms of insects, &c. they avert the moisture which trickles down the necks of those animals whilst they are browsing in rainy weather.

The aquatic Mammalia are not provided with external ears, because no mechanical obstacle could have debarred water from conveying its impurities to their *membrana tympani*, without excluding the water itself, which is their medium of sound, and which, moreover, supplies their ears with a constant lotion.

In moles* and reptiles† an auricle would have been of little avail as a protection against the earth, mud, and dusty rubbish which they frequent; and it would have been at once a hindrance to their motions, and liable to injury. And as the velocity and precision of the flight of birds would also have been impeded by projecting auricles, the orifice of their meatus is covered with feathers, through the interstices of which the undulations of the air are transmitted.‡

Hence it appears that the necessity for an appendage to protect the organ of hearing is commensurate with that for a similar appendage to the organs of sight and smell; and that in different animals the form and properties of the external ear change, or it becomes evanescent, according as the circumstances and habits of the animals render certain adaptations necessary for the purpose of protection, or any adaptation whatever unavailing.

But as we seldom find that the functions of an organ are limited to a single object, so the accessory appendages of the eye and nose have secondary purposes to serve in subordination to their primary office of protecting the organs to which they are attached. The nose has its extremity composed of cartilages moved by their proper muscles, to contract or expand the passages to the nervous papillæ, and thereby to vary at pleasure both the quantity of effluvia admitted, and the velocity with which they pass along the inner surface of the nostrils.§ In the Primates these motions are confined to a narrow range, because the orifices of their nostrils can be readily presented by the rotation of the head itself towards the quarter where the effluvia are strongest.[*] In quadrupeds, especially those with very stiff necks, as the Pachydermata, the nostrils have a much wider field of motion, so that these animals must be able to form a pretty correct estimate of the difference of intensity of effluvia on different sides by only employing the muscles of their snouts, without being constrained to bend their unwieldy necks. In this manner the capacity of the nostrils to increase or diminish the sensation, according as they are placed, either jointly by the rotation of the head, or separately by the motions of the alæ, in more or less favourable circumstances for receiving effluvia, gives a certain power of determining the direction of smells as a secondary function.[†]

---

* In order to supply the want of an external ear, the meatus of the mole is extremely narrow, and its minute orifice is covered with hair, which secures it in a great measure from the admission of particles of earth, whilst the position of the membrane of the tympanum would render it difficult for them to reach it if they were admitted.

† Reptiles have not even a meatus, but their membrane is defended by skin, and in some of them by a cartilaginous plate, which can bear rough treatment.

‡ It is curious to observe how the size of the auricle diminishes in amphibious animals, such as the hippopotamus, the otter, and the seal, as if it were a middle term between its extreme conditions in those confined exclusively to one or other of the two elements which they inhabit.

§ When we wish to enjoy to the fullest extent any fragrant odour with which the atmosphere is imbued, we dilate the nostrils and inspire in large volumes, by which means an increased quantity of effluvia is allowed to rise to the upper part of the nasal cavities, where the organ of smell is placed; when, on the other hand, we wish to detect irritating gases, we contract the nostrils whilst we increase the rapidity of our inspirations, and thus the gas acts with greater effect on the Schneiderian membrane near to the orifice, where. being sensible to pungency but not to fragrance, it belongs rather to the respiratory system than to the organ of smell.

The ears seem to me to possess the very same means of performing this secondary function. The auricles of many animals are so constructed that the intensity of sound may be increased or diminished, according as their orifice is directed towards or from its source. In man and most of the Primates, and also in birds, this is chiefly effected by moving the head; but in quadrupeds, and in some of the Quadrumana, the external ears are endowed with such extensive movements by their own muscles, that a very great difference in the intensity of sounds must result from mere difference of position of the apertures of the funnels;[‡] and this difference of intensity will constitute a sort of acoustic parallax, which will furnish rude data for an instinctive calculation of the places of the sounding bodies.[§]

In short, animals seem to have no other means of judging of the direction of sounds than by marking the difference of their intensity coincident with differences of position of the apertures of their ears, so that in this respect the external ear is strictly analogous to the external nose, both in regard to the mode in which it acquires such information, and the vagueness of the information when acquired.*

---

[*] Even in this class, by the mere alternate expansion of the two nostrils, a certain difference will be observed in the intensity of the sensations excited, which will give a slight indication of the direction of odours.

[†] The eyelids in like manner adjust the quantity of light allowed to fall upon the cornea, and they also increase or diminish the divergence of the rays, according as they contract or expand the chink between their edges. But the ascertainment of direction is an element in the operation of visual perception of too great moment, and requiring too nice precision to be left to an accessory appendage, and it is therefore effected by the voluntary adjustment of the organ of sight itself. This is far from being the case in the sense of smell, which affords very vague indications of the place from whence effluvia are disseminated.

[‡] The hare and other timid animals cannot turn their heads round in the midst of their flight to learn the direction in which their pursuer runs. Their ears accordingly are long and narrow; and they must give considerable power of judging of direction, because the smallest difference in the relative extents of concave and convex surface exposed, either in succession by one ear, or by both ears at one time, will suffice for the detection of a difference in the intensity of the sound.

[§] Moles, reptiles, and amphibious animals require no such provision, because, instead of flying before their pursuers, they betake themselves at once to a medium through which they cannot be followed.

I therefore conclude, that the principal function of the external ear is to *protect* the *membrana tympani* from extraneous disturbing influences; and that, as a secondary function, it indicates, however imperfectly, the *direction* of sounds.

SECTION II.—*On the Mechanical Functions of the Membrana Tympani.*

Much confusion has prevailed in the theory of hearing, from comparing the *membrana tympani* to the parchment of a drum, and conceiving it as vibrating from its tension. Savart's experiments prove, in the first place, that when sounds act upon a membrane of small dimensions, they are incapable of throwing it into nodal sections of vibration; and secondly, that membranes of imperfect rigidity are more easily moved by the undulations of the air, than those which are so tense as to be capable of being excited to vibrate from their elasticity. When the *tensor tympani* is relaxed, the membrane will therefore yield, as a whole, to the undulations of the air; and a smaller force will suffice to communicate to it this passive motion, than would be required to throw it into a state of active vibration.

This, indeed, might have been inferred *a priori*; because, if the *membrana tympani* were excited to vibrate from its own elasticity, in unison with each pitch of sound, it would not only continue to vibrate after the exciting cause had ceased to act, so as to produce a confused singing in the ears, but it would be incapable of vibrating at all, until its tension had been previously adjusted, so as to place it by anticipation in unison with each particular sound with which it was about to be affected.

It would be well, therefore, to banish the term vibration altogether from our description of the motions of the *membrana tympani*, which ought to be regarded as a perfectly passive medium, yielding to each undulation that impinges upon it, but incapable, in its normal† state, of making a single oscillation, in virtue of its intrinsic elasticity, after the impelling force is withdrawn. Even the air itself does not vibrate, except under explosions and similar causes; its molecules are merely pushed forwards by each excursive vibration of the sounding body, and they recede by the pressure of the superincumbent atmosphere, with the returning vibration. In short, the continuance of a sound depends entirely on the elasticity of the sounding body itself, and both the air and the *membrana tympani* are mere passive instruments of communication, which cease to move the instant the sounding body is still.

---

* This explanation, I am aware, is at present out of favour with physiologists; but still it appears to me to be more plausible than any other that has been proposed in its place, and I am happy to find that it has the sanction of Mr Whewell.

† When the ear is assailed by a very sharp and loud sound, as the crack of a whip, a sound still sharper, (by, I think, an octave,) continues after the first sound has ceased. In this case is the *membrana tympani* distended so forcibly by the excessive counteraction of the tensor muscle, that it assumes the action of an elastic membrane?

Now, if we consider attentively the effect of the impactions of the air upon a passive membrane of the figure of the *membrana tympani* in the Mammalia, we shall find that, instead of having its apex pushed farther inwards by each excursive movement, it will be drawn outwards toward the *meatus externus*, in the contrary direction to the action of the tensor muscle, which antagonizes, instead of co-operating with the sonorous transport.

If this membrane were a very extensible substance, admitting of elongation of its radiating fibres by a smaller force than the action of the tensor muscle, the effect of the impulses of the air upon it would be to dilate its superficial dimensions, and to push the whole of it, including its apex, inwards. But if the point of the malleus can be drawn outwards by a smaller force than would be sufficient to elongate the radiating fibres of the membrane, then, I conceive from the configuration of the parts that such will be the actual result.

If the two ends of a string are fixed, and its centre drawn down by a weight appended to it, then by applying pressure downwards to the string at each side of the weight, we raise the weight upwards ;[*] so that impulses upon two of the radiating fibres which connect the handle of the malleus to opposite points of the tympanic ring, ought to draw the handle outwards in the contrary direction to the impulses. If one end of the string is attached to the extremity of a lever which has freedom to move in all directions, and the other fixed to a point above and beyond the lever, then, by pressing the string downwards, we will draw the lever forwards.[*]

---

[*] Let A, (Plate IV. Fig 1,) be the transverse section of a lever, such as the malleus, and let B A C be a string attached to it, and fastened at B and C. If two forces P M and Q N, act simultaneously upon the points M and N, they will be decomposed, and the portions which act perpendicularly to A B, A C, will have a tendency to carry these points along the arcs M R, N S. Let us suppose that the weight W will be raised by a smaller force than would be sufficient to elongate the string, and that the forces P M, Q N, will carry M to R, and N to S. Then the segments B M, C N, will assume the positions B R, C S. From the centres R, S, with radii equal to M A, N A, let two acres be described, meeting each other at O, and join R O, S O. Then since $B R + R O = B A$, and $C S + S O = C A$, B R O S C will be the new position of the string. B A being $= B R + R O$ is greater than B O, and $C A \triangleright C O$. $\therefore$ O is nearer than A to B C, the common base of the two triangles B A C, B O C.

If, instead of being concentrated at M and N, the forces were diffused equally along the whole lines B A and A C, then, instead of forming angles at R and S, the strings would form curves equal in length to B A and A C ; and as the chord of every curve is shorter than the arc which it subtends, the lines B O, O C will be shorter than B A, A C, and O, the vertex of the curve will still fall within the angle A. If the string were not acted on by any other forces besides these impulses, the curve into which it would be thrown would be a catenaria ; but since the weight W acts upon A, pulling that point in the direction opposite to O ; the two segments of the curve will form an angle at Y, between A and O.

I have endeavoured to put these views to the test of experiment, by exposing the *membrana tympani* of a calf, and making a constant, though gentle, pressure at one side of the handle of the malleus,[†] whilst I made slight but quick impressions on the corresponding point of the opposite side with the end of a small probe. By placing the bone in such a position as to obtain a view of the profile of the cone, I distinctly perceived that a very small extent of motion outwards was communicated to the point of the malleus on each push of the probe, and that the tensor muscle was thus put on the stretch.

When the pressure was made on the portion of the *membrana tympani*, which lies beyond and in the same plane with the handle of the malleus, the effect was to draw the apex toward the opposite side of the annular frame; so that the resultant of the two sets of impulses, those at the sides of the bone, and those beyond its point, must be to draw it outwards toward the external meatus, and a little downwards across the aperture of the tympanum.

When pressure is applied to the point of the malleus from within, and in a direction outwards and downwards, it is easily moved to the extent of about a line; but it is moved with difficulty, and to a much smaller extent, in the opposite direction; so that the former may be inferred to be its proper motion. We shall investigate afterwards how such a motion may be reconciled with the form and connections of the malleus.[‡]

---

[*] Let a string A D (Fig 2,) be fastened at D in the same plain with the lever E A; let D A be thrown into a curve, of which D V is equal to the chord; and let a smaller force be sufficient to raise the weights W and Z than to elongate the string. From centre D with radius D V, describe the arc F V G; the new locus of A will be somewhere in that arc. Let A H be the direction of the motion of the lever due to the impulses along the string formerly considered (p. 53, note); let A V be the direction of the motion due to the impulses we are now considering; and let A K be the direction of the resultant of these two motions; then O, the point of intersection of the arc F. G, and the diagonal A K, will be the new position of A. The weights Z and W represent the actions of the muscles of the malleus.

[†] When the impulses were applied at one point without counter-pressure at the opposite side of the malleus, the effect was to draw the extremity of the bone towards the side where the probe was applied; but when the opposite side was steadily fixed, the impulses, if sufficiently slight, drew the apex perceptibly, though to a very small extent, outwards. When the pressure was too great, and not perpendicular to the surface, it extended the portion of the membrane behind it without acting on the part between it and the apex, and the result was a dragging inwards of the whole apparatus. The experiment requires, therefore, to be conducted with nicety, and the pulses to be made with a very short and quick movement, perpendicular to the membrane.

[‡] In birds the apex of the cone of the *membrana tympani* points outwards; and the impulses of the air upon the small segment that is traversed by the external branch of the ossiculum would have the effect of pushing the apex directly inwards, if not influenced by the action of that bone as a crooked lever. But so large a portion

SECTION III.—*On the Mechanical Functions of the Ossicula of the Tympanum.*

Such impulses on the *membrana tympani* as are sufficient, though slight, to cause a visible motion outwards and downwards of the point of the malleus, produce so little effect on the stapes, that the direction of its motion cannot be easily determined by the naked eye. In order to ascertain this, it becomes necessary to extend the range of motion of the malleus by making pressure on its point from the cavity of the tympanum. I obtained a view of the stapes by sawing off a portion of the temporal bone of a calf through the mastoid cells; and on pushing the point of the malleus with a probe outwards and toward the opposite side of the ring, a motion became obvious at the stapedian joint of the incus, the direction of which corresponded with that of the point of the malleus, viz. obliquely outwards and downwards. By this motion the base of the stapes was drawn away from the *fenestra ovalis*, its upper margin * describing a longer arc than the lower; the effect of which would be to cause the stapes to act upon the water of the labyrinth by suction, like the piston of a pump, and not by pressure, according to the prevailing opinion.

From these motions of the two extremities of the chain of ossicula, which have been ascertained by observation, those of the intermediate parts may be easily deduced.

The centre of motion of the malleus is its styloid process, which rests upon the temporal bone close to the tympanic ring. It is obvious that this process may both describe a circle around its point, and roll upon its longitudinal axis; so that the extremity of the handle will be capable of describing arcs in two planes; in one it may be carried downwards, toward the opposite side of the ring; in the other outwards, toward the external meatus; † and the resultant of these two motions will be in a curve stretching obliquely downwards and outwards. As we have seen that

---

of the membrane in birds lies beyond the point of the bone, that the effect of the lateral segments may be disregarded; and I have found that, on pressing the points of two probes simultaneously on different parts of the large segment of the *membrana tympani* of a hen, the apex moved downwards and a little inwards. In moles the *membrana tympani* appears to be a plane when the animal is dead; so that this is probably its quiescent condition; and we may assume that, when the mole leaves the earth, and requires to hear through the atmosphere, it will throw its tensor muscle into action, and thereby draw the point of the malleus slightly inwards, as in other animals of the same class.

* Indeed, I question whether the lower margin does not remain *in situ*, and serve as an axis for the upper; but this motion is so minute that it is difficult, even after laying open the vestibule, to ascertain its precise nature.

† Let A B (Fig. 3) be the styloid process, C the articular extremity, and D the point of the handle of the malleus. From centre A describe D E; D will move in this curve downwards and slightly forwards; but it may at the same time perform rotation around A B as an axis, by which D will be brought outwards. The whole of the handle, neck, and head of the malleus occupy fully a sextant of the circle, so that if D moves downwards, C will move a little backwards around the centre A.

this is precisely the direction due to forces acting simultaneously from without over the whole of the *membrana tympani*, we now find that the action we have assigned to that membrane is compatible with the conditions of the malleus.

The incus has motions upon its long crus exactly similar to those of the malleus upon its styloid process; and as these radii are in different planes, the two bones with their facettes may be compared to two wheels touching each other obliquely, and whose circumferences are mutually indented, so that the rotation of the one will communicate motion to the contiguous arc of the other in the same direction. When the point of the malleus moves downwards and outwards, the stapedian joint of the incus will accordingly do the same.[*]

Lastly, since the short crus of the incus, or rather the chord of its curve, makes an obtuse angle with the plane of the crura of the stapes, the oblique movement of the former will render this angle less obtuse; and in proportion as it approximates to a right angle, the base of the stapes will tend towards parallelism with the incus, which implies that its upper margin shall be drawn outwards from the *fenestra ovalis*.[†]

But besides the direction of these motions, their extent and momentum are important subjects for investigation.

A perpendicular from the point of the handle of the malleus to its styloid process, and also a radius to the same point from the extremity of that process, are respectively greater than a perpendicular, and a radius from its articulation with the incus; and, therefore, the rotatory motion of the handle around the styloid process as an axis, as well as its circular motion around the point of that process as a centre, will both have a larger range than the corresponding motions of the articulation of the malleus with the incus. But the reverse of this takes place in the motions of the incus, because both the point and axis of its long crus are nearer to its stapedian than to its mallear joint. Hence, each of the elements of the compound motion of the handle of the malleus outwards and downwards, suffers two diminutions in passing to the summit of the stapes; and a farther reduction takes place in proportion of the height of the crura of the stapes to the breadth of its base, in transferring the ultimate effect to the *fenestra ovalis*. The undulations of the air, therefore, undergo three diminutions in point of range, before they take effect upon the water of the labyrinth; and if we assume that the compound ratio of the diminution is as 8 : 1, (which, from studying the relative dimensions of the parts, I suspect to be near the truth), then, whatever weight is applied upon the *membrana tympani*, will be capable of sustaining eight times that weight at the base of the stapes.

---

[*] Whilst C F B (Fig 4), the common surface of articulation of the malleus and incus, will move downwards and a little backwards around A as a centre, and inwards around A B and B G, as axes, the stapedian joint H will move outwards, and at the same time downwards.

[†] The motion of H (Fig. 4) around I as a centre will carry K outwards.

There is, however, another force besides the impulses of the air which acts upon the apparatus of the tympanum, viz. its voluntary muscles. The *tensor tympani* draws the handle of the malleus directly inwards; the *laxator tympani* (as the second muscle is erroneously called) draws it upwards; and the stapedian muscle draws the upper margin of the base of the stapes inwards upon the *fenestra ovalis*. These muscles, therefore, exactly antagonize the effects of the sonorous impulses, and restore all the bones and membranes to their quiescent positions. And this is precisely analogous to the operation of the voluntary muscles of touch, which react upon the object producing the sensation.

We have hitherto supposed that the primary impelling force is applied to the *membrana tympani*, and transferred from thence to the ossicula; but vibrations may be conceived to exist in the temporal bone, and impulses to be imparted from it to the points of the styloid process of the malleus, and of the long crus of the incus. In this case, the extremity of the handle of the malleus will become fixed, and serve as the first fulcrum,* whilst the resistance will be at its joint with the incus. If an impulse is also received through the long crus of the incus, the second centre of motion will be between it and the mallear joint; and the stapedian joint will have a greater range than either of the actuating forces. A smaller weight at the base of the stapes will, therefore, hold in equilibrio a larger at the points of the long crus and styloid process.†

SECTION IV.—*On the Mechanical Functions of the Water of the Labyrinth.*

Sound, like light, loses a portion of its intensity by passing from one medium to another; and this is equally the case, whether it passes from a rarer to a denser medium, or the contrary. The loss takes place at the surface of contact of the two media. Let air, for instance, be the medium through which the vibrations of a sounding body are made to act upon water. Each molecule of the air in contact with the sounding body will oscillate forwards and backwards over precisely the same extent of space with the molecules of the sounding body itself: and as each stratum of molecules will communicate its motion to the stratum beyond it, the sound will be conveyed to the surface of the water, suffering a slight retardation from the interstitial compression of the air. As we suppose that the molecules of the sounding body act entirely by pressure,* they must give a somewhat smaller velocity to the particles of air than they would have themselves, if placed in a vacuum; and, therefore, the momentum of the undulations of the air will be less than that of the vibrations proper to the sounding body in a somewhat greater ratio than the inverse ratio of their respective densities. But there will likewise be a loss of momentum in transmitting the motion from the air to the water, because when air impinges on water, it is not capable, from its inferior density, of giving the same velocity to the water that it had itself. The motion of the stratum in contact with the water is therefore checked, and the different strata are condensed upon each other until

---

* When we apply moderate pressure over the orifices of our ears, we can hear the friction of the point of the tongue upon the teeth, which was inaudible when the ears were patent. The support given to the point of the malleus by the air compressed in the meatus affords a firmer fulcrum than when the air was free. But if we apply the pressure with extreme violence with the point of a finger, the sound becomes less distinct; because the force of the pressure has been such as to impede the lever action of the entire handle of the malleus.

† On pushing the point M, (Fig. 5,) of the *membrana tympani* of a bird to R, the locus of A will be somewhere in the arc O P, described from centre R with radius M A. From centre F, with radius F A, describe an arc, and O, the point of intersection of these two arcs will be the new position of A. The ultimate effect will be to raise S to T. So that although the convexity of the *membrana tympani* of birds causes its apex to be pushed a little inwards by impulses of the air, the nature of the lever motion of the ossiculum raises the upper edge of its base from the *fenestra ovalis*.

---

* Let B = specific gravity of a bell,
A = do. do. of air,
W = do. do. of water,
V = velocity of vibration of B.

If B strikes A, and moves along with it the velocity of B + A = $\frac{BV}{B+A} = V\frac{1}{1+\frac{A}{B}}$

i. e. the velocity communicated to A is less than that belonging to B in the proportion of $\frac{1}{1+\frac{A}{B}}$: 1, and the momentum of B : momentum A :: B V : A V × $\frac{1}{1+\frac{A}{B}}$

If A strikes water with velocity $=\frac{V}{1+\frac{A}{B}}$, and moves along with it, the velocity of A + W $= \frac{A}{A+W} \times \frac{V}{1+\frac{A}{B}} = V \times \frac{1}{1+\frac{A}{B}+\frac{W}{A}+\frac{W}{B}}$, which shows a still farther reduction of velocity in passing from air to water.

Suppose specific gravity of A = 1
of W = 827
of B = 10,000,

the velocity communicated from B, through A to W =

$$V \times \frac{1}{1+\frac{1}{10{,}000}+827+\frac{827}{10{,}000}} = \frac{V}{828\frac{1}{12}} \text{ nearly.}$$

they obtain space for re-expansion by the retiring vibration of the sounding body, or by communicating motion obliquely to the surrounding air. The undulations of the air are therefore partly stifled and partly reverberated by their own condensation, which employs a portion of their momentum; and it is only the remaining portion that can take effect upon the water. This waste of intensity is at the same time attended with a derangement in the uniformity of velocity throughout each undulation; for instead of being communicated at once at its full rate to the new medium, a delay takes place, during which the air is acquiring a gradually increasing elastic force, the effect of which upon the water must also gradually increase till it attains its maximum. It is easy to conceive that this might interfere most essentially with the power of discriminating the qualities of sound.

But as in the transmission of light advantage may be taken of difference of density for concentrating the rays received over a large surface upon a small focus, in order to augment their intensity; so in sound the particles of air may be made to impinge upon a large surface which they can easily move, and not only have their united forces concentrated upon a smaller spot, but their velocity reduced, while their momentum is preserved so as to adapt* them to the difference of density between air and the new medium to which they are communicated, and thereby to prevent both loss of power and loss of precision from condensation. If the area of the *membrana tympani* were to that of the *membrana ovalis* as 100 : 1, and if the whole impulses on the former were made effective upon the latter, the undulations would be transferred without loss from condensation to a fluid a hundred times more dense than the air, because the weight is increased a hundred-fold by the superior extent of the recipient surface. If again, the range of motion of the *membrana tympani* is to that of the *membrana ovalis*, as 8 : 1, the impulses of the air upon the former will be capable of communicating the whole of their momentum to a fluid eight times denser than the air. By combining these two ratios, the total effect will be to transfer the momentum of the undulations of the air impinging upon the *membrana tympani*, without either waste or remora from condensation to a fluid 800 times more dense than the air itself.

Now, this is nearly the ratio of the specific gravity of water to that of air; and it becomes a most interesting problem to demonstrate the precise results of the chain of levers presented by the ossicula of the tympanum, and the comparative areas of the membranes of the tympanum, and of the *fenestra ovalis.* Although I have only made a rough attempt to supply these important desiderata by actual measurements, I shall, in the meantime, assume that nature has shown here the same economy of power that is displayed in all her works; and that the mechanical adaptations are such that there is no waste in transferring the impulses of the air upon the *membrana tympani* to the water of the labyrinth.

---

* Let the discs A, (Fig. 6,) and B be suspended in air and water, respectively upon the fulcrum C, and let their areas be to each other inversely as the specific gravity of air to the specific gravity of water; and let $CA = CB$. Then, if particles of air impinge over the whole of A, they will give their own velocity to the water in contact with B.

Let $A = B$, (Fig. 7,) and $CB : CA :: $ sp. gr. air : sp. gr. water; then A will still hold B in equilibrio, but it will give it a smaller range of motion.

Let $B < A$, (Fig. 8,) but not in the ratio of the specific gravities of air and water; and let $B \times CA = A \times CB$; still the equilibrium will be maintained.

The perilymph, then, is put in motion by the suction of the base of the stapes; and the pressure of the atmosphere through the Eustachian tube, upon the *fenestra rotunda*, is an essential element in this mechanical operation. As we know that loud sounds are those which result from extensive vibrations in the sounding body, we may presume that the intensity of the sensation will depend on the extent of oscillation of the perilymph over the surface of the membranous labyrinth, which we shall consider as the immediate organ of hearing. Let us suppose that the stapes has been raised from the *fenestra ovalis* by an excursive undulation of the air; then the perilymph will be sucked out from all the channels of the labyrinth, and a general conflux will take place towards the *fenestra ovalis*; and, in order to supply the space thus vacated, the membrane of the *fenestra rotunda* will be pressed inwards by the weight of the atmosphere. In this course, as much water must pass from the tympanic scala of the cochlea, through the helicotrema, as is pumped out by the stapes. When the oscillation retires, the atmospheric pressure upon the two fenestræ will be equalized, and the stapedian muscle will force back the perilymph through the same passages by which it advanced, till the *membrana rotunda* is restored to its quiescent state. The action of the sonorous undulations, and the reaction of the acoustic muscles are therefore two forces which produce alternate fluxes and refluxes of the perilymph over the membranous pulps supporting the delicate expansions of the auditory nerve; and this seems to be the essential condition for exciting the sensation of sound.*

The resemblance of the cochlea in structure and relative situation to the iris has attracted the notice of anatomists; and it is not an idle speculation to inquire whether the same analogy may not subsist between their functions. When the light falls with great intensity on the surface of the iris, that membrane expands so as to contract the pupil and diminish the quantity of rays passing to the retina; and it appears probable that this expansion is not so much the effect of muscular action, as of vascular congestion; so that there is not merely a dilatation, but a positive enlargement of the iris. In like manner, we may suppose that the friction of extensive oscillations along the scalæ of the cochlea causes a similar excitement of its spiral laminæ, a flow of blood is determined to their erectile tissue, and both the channels of the scalæ and the orifice of the helicotrema are narrowed. An impediment being thus thrown in the way of the movements of the perilymph, the extent of its undulations comes to be regulated by its own action.

---

* As it is only the distinctness of visual perception, and not the sensation of light that is destroyed by removing the lens of the eye, so the loss of the ossicula and membrane of the tympanum does not necessarily entail destruction of hearing so long as the stapes and its muscle are preserved. The air may impinge directly on the *membrana rotunda*, and raise the base of the stapes, which will be replaced by its muscle; and thus oscillations will be created over the membranous pulps, though less forcibly and less precisely than if transmitted through the ossicula.

But when a flow of blood is brought from the general circulation to fill the erectile tissue of the cochlea, the labyrinth would suffer from over-distension, and a protrusion of the membranes of the two fenestræ, impeding their functions, would be the result, unless some provision were made to obviate this evil. Such a provision seems to be found in the aqueducts of Cotugno, one opening at each scala of the cochlea, by which the perilymph displaced by the turgid laminæ is conveyed out of the cavity of the internal ear, and placed within the cranium. The force by which it is expelled is the excess of the erectile action of the cochlear circulation, over the normal action of the encephalic circulation: and when this excessive action terminates and the equilibrium is restored, the normal action within the cranium becomes sufficient to replace the perilymph.

The retina of the eye is defended from the admission of such rays as have not been previously concentrated by passing through the cornea and lens, by means of the *pigmentum nigrum*, which darkens the internal chamber of the eye, by preventing translucency through the sclerotic coat. In like mannner a provision has been made for preserving the auditory nerve from any vibrations except those which have acquired the necessary precision, from being conveyed to the *fenestra ovalis* by the ossicula of the tympanum. The bones of the cranium are excellent conductors of vibrations received from solid bodies. The voice is conveyed by the vibrations of the laryngeal cartilages to each cornu of the os hyoides, and from thence along a tense ligament to the styloid process of the temporal bone. From this they are communicated to the spongy and cellular portions of that bone, particularly its mastoid cells, the laminæ of which resemble a system of sounding boards. We have seen how the malleus and incus are connected with the temporal bone, and how their lever action is adjusted so as to adapt the vibrations of the solid bone to the fluid perilymph.

But if these vibrations had likewise access to the perilymph through the walls of the labyrinth, the ear would be filled with a confusion of vibrations, the direction of which would have no relation to the regular and precise flux and reflux of the perilymph over the auditory pulp; and the ear would be placed in the same predicament that the eye would be in, if the retina were exposed to a glare of light through the sclerotic coat which had not been previously brought to a focus by passing through the cornea and lens.

Now it appears to me that, as the *pigmentum nigrum* is provided for darkening the eye, so the petrous bone is provided for deafening the ear. Its extreme density, compared with that of the cellular bones by which it is surrounded, will serve to stifle the sounds. The density of this bone cannot be intended as a protection to the ear, because the situation of that organ exempts it from external violence; and animals having no larynx, and therefore no voice, have no petrous bone, although their internal ears stand as much in need of protection as those of animals endowed with a larynx.

Summary

1. The external ear protects the *membrana tympani*, and at the same time contributes to a knowledge of the direction of sounds.

2. The *membrana tympani* presents a large recipient surface for sonorous impulses, which draw its apex outwards.

3. The ossicula of the tympanum constitute a system of levers by which the motions of the *membrana tympani* are transmitted to the *fenestra ovalis*, with their extent diminished but their momentum preserved.

4. The laminæ of the cellular bones which surround the ear convey vibrations from the larynx by means of the ossicula to the *fenestra ovalis*; and these have their extent increased whilst their momentum remains the same.

5. The combined effect of the difference of extent of the *membrana tympani* and *fenestra ovalis*, and the difference of range of motion of the point of the malleus and base of the stapes, is to concentrate the impulses of the air, and adapt them for being transmitted without loss to a denser fluid.

6. The stapes is raised from the *fenestra ovalis* by the impulses of the sonorous undulations.

7. The muscles of the tympanum restore the *membrana tympani*, and the base of the stapes to their quiescent positions.

8. The *fenestra rotunda* permits the pressure of the atmosphere to act through the Eustachian tube upon the water of the labyrinth, so as to enable the stapes to be raised.

9. The water of the labyrinth oscillates over the auditory pulps

by the alternate suction and pressure of the stapes, and this excites the sensation of hearing.

10. The extent of oscillation is regulated by the cochlea, the laminæ of which expand and contract according to the degree of friction to which they are exposed.

11. The aqueducts are diverticula for receiving the water displaced by the expanded laminæ.

12. The petrous bone deafens the internal ear by excluding such vibrations as are not transmitted by the ossicula.

Ayr, 17th September 1840.

Art. V.—*Case of Aortal Aneurism which opened into the Vena Cava Superior, opposite the entrance of the Vena Azygos.* By William Young, M. D. [Read to the Medico-Chirurgical Society of Edinburgh.]

A gentleman, aged 56, of rather robust constitution and temperate habits, had enjoyed good health till autumn 1838, when he suffered under an attack of rheumatism, seated chiefly in the shoulders, occiput, and front of the thorax, with pain extending from the upper part of the sternum to the spine. These pains continued with greater or less severity for some months, but he complained most of the pain in the occiput and internal part of the chest. He remained free from pain during the following summer till October, when there was a partial return, but less severe than formerly. He was at this period attacked with cough, loud and ringing, accompanied with scanty expectoration, and most troublesome when in the recumbent posture. This occasioned his confinement to the house for some weeks. He never at any period complained of dysphagia, palpitation, or any sense of constriction or oppression of the chest, nor experienced difficulty in breathing, even on taking violent exercise ;—a symptom almost always present in this disease, especially in its advanced stage. Neither was there ever observed any elevation or swelling of the jugular veins, or preternatural pulsation of the carotids. But he had for many years suffered from terrific dreams, which he attributed to derangement of the digestive organs.

On the evening of the 8th of January last, he retired to bed complaining of headach, to which he had been occasionally subject for many years.

Not more than an hour after when I saw him, the lips, nostrils, and features generally, as also the head, neck, and front of the chest as far as the second rib, were greatly swollen, and of a deep purple hue, approaching to black, which partially disappeared on pressure. The veins of the temples and forehead were black and prominent. He complained of severe headach, but no difficulty in breathing, nor were there any symptoms of oppression of the brain. The pulse was 80, and full. The disease appeared to be anomalous in its nature. It had none of the characters of emphysema or erysipelas. Blood was drawn from the arm to the extent of thirty ounces, it being obvious that some serious obstacle to the return of the blood from the head existed. After the bleeding the deep colour of the surface, as well as the pain, were considerably diminished.

January 9. The fulness of the face, &c. and purple colour of the skin were as when first visited, and he complained still of severe headach; the respiration was rather difficult; the pulse 80, small, and easily compressed. He was very restless, and constantly changing his posture, and had the appearance of great suffering. The examination of the chest in such circumstances could not be made with all the necessary minuteness, yet the respiratory sound could be heard over the greater part of the chest anteriorly. The action of the heart was natural; the impulse rather feebler than usual between the ribs; the sounds distinct, but not loud, and rather obscured by the bronchial sounds.

Over the centre of the sternum there was distinctly perceived an impulse stronger than that of the heart accompanied by a short, rather abrupt and loud sound, but no distinct bruit. The impulse was limited in extent, diminishing in intensity as the stethoscope was applied at a greater distance from this point. No purring tremor or rasping sound could be heard either at the upper part of the sternum or above the clavicles.*

From the extremely restless state of the patient, and the chest having since the commencement of the attack become œdematous, percussion was not employed.

No further opportunity of making another examination was afforded, as the patient generally lay in such a position as to prevent any auscultation, except of the back, where the respiration was obviously becoming more and more bronchial with loud crepitation.

Jan. 10. Breathing was more difficult, obviously in consequence of the obstruction to the free ingress and egress of the air at the upper part of the trachea, the effect of the external swelling extending to the fauces and epiglottis. Deglutition was much impeded from the same cause. Twenty leeches applied externally gave no relief.

---

* Dr Abercrombie and Dr John Scott visited the patient and employed the stethoscope with the same result. As far as the examination of the chest could be conducted in this case, no difference in the respiration of the two lungs could be detected; noticed particularly as a diagnostic symptom of aneurism of the aorta by Dr Green and Dr Stokes in their papers on this subject in the Dublin Medical Journal for 1835.

January 11. He talked incoherently. The respiration was extremely difficult; complete dysphagia, which was always referred by the patient, while able to speak, to the upper part of the œsophagus, and not as usual in such cases, to some point nearly opposite the spot where the impulse is perceived. He was apparently in great suffering, not remaining more than a few minutes in the same posture.

Died at 4 P. M. Immediately after death, the surface assumed its natural colour, and the fulness of the parts greatly subsided.

The body was inspected on Monday, the 13th of January 1840, forty-five hours after death, in presence of Drs Abercrombie, Handyside, and John Scott, and Mr Alexander and Mr Millar. Dr Handyside conducted the inspection.

The surface of the body presented the ordinary appearances, with the exception of some lividity of the features, existing to a greater extent than is usual at the expiry of so short a period after death, and a mottled appearance on the right mammary region, arising from venous turgescence, and some ecchymosed spots in the texture, and under the substance of the skin.

On dividing the integuments of the neck and chest, a considerable quantity of serum escaped from the interstices of the skin and subcutaneous textures. The thorax immediately below the jugular fossa, and ridge of the collar bone, appeared to be rather more prominent than usual, and the anterior surface of the sternum was remarkably concave in its long axis. The cartilages of the first ribs were in a completely ossified state, while those of the other ribs approximated to this condition.

On removing the sternum and the cartilages of the ribs, the anterior cavity of the mediastinum appeared to be very nearly obliterated, the anterior wall of the *pericardium reflexum* adhering intimately to the adjacent surface of the sternum. The internal surface of the pericardium, with its fluid, were natural; but the heart lay collapsed, and nearly empty of blood, its parietes (not hypertrophied, as usual in this disease), but soft, flaccid, and of a dark-grayish hue. The cavities and the valvular apparatus of this organ were healthy, with the exception of the aortal aperture and its valves. The former was dilated to the extent of a half-crown piece, and the latter were enlarged and thickened. The ascending aorta was enormously dilated, and in conjunction with the arch of that vessel formed a large aneurismal sac, the long axis of which lay transversely, and the capacity of which amounted to about a pound and a quarter. This cavity was filled with blood principally in a fluid state, but there lay imbedded in, and closely adherent internally to the posterior part of its right wall, at a part corresponding to that along which the *vena cava* superior descended externally, a circular coagulum of rather recent formation, the edges of which were fimbriated and uneven, the centre of it thick and convex. A similar coagulum, but of smaller size, lay in another recess, adherent to the inner surface of the anterior wall of the sac. At these two parts, the walls of the sac did not exceed a line in thickness, while internally they were of a cherry-red colour, and exhibited vascular concretions with the earliest-formed laminæ of the coagula. The remainder of the inner surface of the sac presented in some parts of its texture an atheromatous degeneration, which at the back part of the sac seemed, in conjunction with the altered lining membrane, to be broken down into numerous flocculent appendages, which projected into the cavity. In other parts, the texture of the aneurismal sac was pervaded by incipient calcareous depositions.

The orifices of the coronary arteries and of the branches of the arch of the aorta were slightly dilated.

The walls of the aneurismal sac amounted on the average to two lines and a half in thickness, while at the part where the *vena cava* superior lay stretched over, and inseparably connected to the convex surface of the tumour, and opposite that part of the course of this vessel, where the *vena azygos* terminates in it, the wall of the sac was much attenuated, being reduced to less than one line in thickness. Here was found an elliptical ulcerated opening in the right wall of the aneurism, and in the tense wall of the vein. The length of this opening exceeded half an inch, and its long axis lay in the direction downwards, forwards, and inwards. Its edges were retracted and irregular, and reduced to half a line in thickness.

The lungs were healthy in texture; but congested with dark-coloured fluid blood.

It will be remarked that in this case there was no symptom of aneurism previous to the attack on the night of the 8th of January. The patient had never even complained of difficulty of breathing, or oppression of the chest. No cerebral affection had ever manifested itself, if we except occasional headachs and a liability to frightful dreams, which he justly attributed to derangement in the function of digestion, having been for thirty or forty years afflicted with dyspeptic symptoms.

The very dark colour and swollen state of the face, head, and neck, appeared to be the result of the engorged state of the cava, produced partly by the mechanical pressure of the aneurismal sac, but chiefly by the profuse discharge of blood from the latter into the vein, immediately on the communication being formed; although œdematous intumescence of the face and even apoplexy were found to result from simple pressure of the aneurismal sac on the cava, as noticed by Corvisart, Bertin, Bouillaud, and Dr Hope.

No case exactly similar to this is recorded by Morgagni. One is mentioned by him, where the skin of the face and neck were livid before death. He says, "the thorax being opened, and water being found therein, so great a dilatation of the heart was observed as had never been seen before, especially on account of the enlarged state of the right ventricle, and its annexed auricle. And the aorta was contracted to an amazing narrowness near the heart." Another, where he says, " the face was tumid and of a brownish red." After death the face continued " turgid and livid." On examination, aneurism of the aorta was discovered exceeding that of a " kid's head, extending from the basis of the heart to the upper part of the thorax."

A case, where the symptoms were somewhat similar to the above, is detailed by Dr John Reid in the January number of this Journal for 1840, where the aneurism communicated with the right auricle, in which there was lividity and œdema of the upper parts of the body, which he attributes to compression of the superior cava. In the present case it is presumed that the simple mechanical pressure of the tumour could not account for the symptoms; these having appeared instantaneously, while there is no evidence of any sudden increase of the size of the aneurismal sac. It is probable that the communication with the vein was formed on the evening of the 8th, when the symptoms first appeared, and which, as formerly stated, would necessarily have the effect of permitting a large quantity of blood to be forced into the vein, and thus produce an immense accumulation of blood in that vessel, and consequently in those of the head and neck.

Art. VI.—*Two Cases of Impracticable Labour arising from Malacosteon of the Pelvis, in which the Cæsarean Operation was performed; accompanied with practical observations.* By Thomas Radford, M. D., Fellow of the Royal College of Physicians of Edinburgh, &c. &c.

Case. I—On Sunday the 1st of April 1820, I was requested to visit Mary Ashworth, residing at Denton, about six miles from Manchester. I was told she was in great danger, having been in labour a considerable length of time, and that no progress was made in the case. This report did not surprise me when I ascertained who the individual was; for Mr Wood, my partner and esteemed relative, had visited her about the end of the seventh month of her present pregnancy, at the request of her medical attendant, Mr Morris, a highly respectable surgeon, who resided at Ashton-under-Lyne. Mr Wood at this period examined her *per vaginam*, and his opinion was, that, if her pregnancy did proceed, when labour came on, the Cæsarean section would be required, as in her case no other means would be of the least avail. At 3 o'clock P. M. I reached her dwelling, and found Mr Morris and Mr Cheetham awaiting my arrival.

I was informed by Mr Morris that she had been in strong labour about thirty-four hours; that the membranes had ruptured in two hours after its commencement; and that the *liquor amnii* had gradually passed away. He had not been able to feel the presentation nor the *os uteri*. The pains were strong for twenty-four hours, but afterwards gradually abated. The urine had been passed freely during the Saturday; but this day (Sunday) there was no evidence of any having been discharged. The bowels were constipated, and had not been opened during the labour.

Her previous history was to the following effect. She had borne ten children, nine of whom were expelled by the natural powers. In the last labour considerable difficulty occurred, and the practitioner had recourse to craniotomy. During her tenth pregnancy she experienced considerable weakness in her loins, and felt rheumatic pains about the hips, and limped in her gait. These pains continued from that time till her present pregnancy, but did not increase in degree. When she became again pregnant her sufferings increased, and her lameness became more manifest. Her stature was now observed to diminish in height. She was 42 years of age, and was employed as a hat-trimmer.

I found her in bed lying upon the back, with the head and shoulders raised. She moved with the greatest difficulty. The pulse was feeble and frequent, beating about 150 in the minute. She had often vomited, and had great tenderness in the belly, which was considerably increased by pressure. Her tongue was furred and dry, and she complained of great thirst; her countenance expressed considerable anguish. Being requested to compose her mind, she answered, "she was composed, but anxious for relief, and would suffer any pain so that she might be delivered."

Upon examining the abdomen, I found the uterus projecting very much forwards, and lying with its anterior surface upon the upper part of the thighs. By a vaginal examination I discovered that the labia were much swelled, and the vagina felt dry and rough; it was hotter than natural; and an odour similar to that arising from animal matter when partially decomposed was perceived from the hand when it was withdrawn. The outlet of the pelvis had undergone great change; the arch of the pubes was totally destroyed by the near approximation of the rami of the ischia and pubes, having only a small slit, so narrow at the upper and lower parts as not to admit the point of the index finger; at the middle, however, the finger could just be introduced. The tuberosities of the ischia were not more than one inch and a-half to one inch and three-quarters asunder; and the lower portion of the sacrum was so much more incurvated than natural, as to throw the coccyx much more forward, and consequently lessen the conjugate diameter of the lower aperture of the pelvis. This great diminution in the outlet rendered it difficult to pass the hand in order to measure the brim, and it was found necessary to carry it very far backwards in order to accomplish it. This aperture was found much more altered than the outlet; one finger only edgeways could be placed between the points of bone in the conjugate diameter. In traversing it from side to side I could detect no great difference, but if there was any, the left was the most contracted. In the transverse diameter I could just place three fingers parallel to each other. The figure of the brim was tripartite, having a slit on each side; and a third passing forwards produced by the approximation and jutting out of the pubes, which was so narrow that the finger could not pass within it. This alteration in the brim was occasioned by the falling downwards and forwards of the upper part of the sacrum, and the lower lumbar vertebra, and by the body of the *ossa pubis* and ischia being forced backwards and inwards, whilst the symphysis and rami of the pubes projected forwards and upwards. The measurement of the conjugate diameter did not exceed three-quarters of an inch, or that of the transverse two inches; and having placed my fingers upon each other in the widest part, and having measured them when withdrawn, I concluded that no body of a diameter greater than from three-quarters to one inch could pass through it; and that delivery *per vias naturales*, aided by the crotchet, was utterly impracticable. Another important feature in the case was, that no part of the child or *os uteri* could be felt.

Upon these grounds, then, we concluded that our only resource was the Cæsarean section. Our opinions were now stated to the husband and friends, and they cheerfully submitted to any practice we thought best to adopt. The patient, anxious to have her sufferings terminated, also readily acquiesced in our decision.

An enema was ordered to be administered, and it soon operated. The catheter was also introduced, but little urine was withdrawn. As the patient felt cold, a little warm wine and water was given, which acted beneficially. Having placed her upon a table, an incision of six inches long was made through the abdominal integuments about one inch to the left of the umbilicus, extending from three inches above to three below. A small opening was made into the peritoneum, and this membrane was afterwards fully divided by a probe-pointed bistoury. The uterus was now exposed, and an excision of equal length was made into this organ, nearly dividing its entire substance. An opening was now made at the lowest point of the wound by the knife, so as to admit the finger, upon which the bistoury was again passed, and the uterus was laid open. I now passed my hand, and, taking hold of the thigh of the child, readily extracted it; but unfortunately it was dead. The funis having been divided, the placental portion was held firmly in the left hand, whilst the right was introduced into the uterus to extract the placenta, which was attached to the upper and posterior part of the uterus. As soon as the placenta was removed, the uterus energetically contracted, and lowering itself became almost invisible. The intestines protruded at the wound, but were soon reduced and retained by the hands extended over their surface. Mr Morris next passed several ligatures through the abdominal parietes, and afterwards applied slips of adhesive plaster, by which the edges of the wound were closely approximated. Pledgets of lint spread with cerate were also applied, and in order to secure the whole a broad bandage was loosely put on. The quantity of blood lost was trivial, not exceeding three or four ounces, which favourable circumstance, doubtless, was partly owing to the position of the placenta, and partly to the vigorous contraction of the uterus.

During the whole course of the operation the patient maintained the greatest fortitude, and expressed her thankfulness upon the termination of her sufferings. She, however, as well as all present, were disappointed that the child was lost.

The patient was then put to bed, and, as the pulse was rather low, a cordial was administered, which in a little while revived her. An anodyne draught, containing 60 minims of laudanum, was also given.

At ten o'clock P. M., the pulse was 140; the skin was hot; she was thirsty, and complained of headach; belly tender; discharge not more than usual; thinks she can sleep; no urine passed.

April 2d, 8 o'clock A. M. Says she has slept; has taken some refreshment; skin hotter; pulse 140 to 150 in the minute, and feels sharp to the finger; has not voided any urine; bowels not moved. Ordered saline effervescing draughts. The catheter to be introduced, by which from three to four ounces of urine were withdrawn.

12 o'clock noon. Not so well; had shivering and some vomiting; the pulse was more frequent; and the abdominal tenderness increased; belly swelled; a sanious discharge was oozing from the wound; bandage uncomfortable; vaginal discharge rather greater and more offensive; bowels not moved. The medicines were continued; and the bandage loosened.

4 o'clock P. M. Has again shivered; continues to vomit; pulse more frequent and tremulous; countenance more depressed; abdomen more tender and more swelled; is very thirsty; and her tongue is very much loaded. The bowels still constipated. The saline medicines were continued, and an enema with oil of turpentine and castor oil was ordered to be administered.

10 o'clock P. M. Vomiting continues unabated; pulse still very frequent and much weaker; skin colder and rather clammy; is slightly incoherent; belly very tender and much swelled; discharge offensive; has not passed urine; the enema operated. The catheter introduced, and four ounces of urine withdrawn; the bandage still further loosened. To have a little brandy in her

gruel; to take 40 minims of laudanum. The symptoms continued to grow more unfavourable during the night, and she died at 4 o'clock this morning, (Tuesday,) about 35 hours after the operation.

An application was made to examine the body, but permission was only granted under a promise that the wound was alone to be inspected; but Mr Morris while alone took the opportunity of ascertaining, as far as he was able, the state of the parts. The edges of the external wound were quite separate, and had a flabby unhealthy aspect. Having divided the stitches and drawn aside the integuments, the uterus was observed to be well contracted. The wound was much diminished, its edges were loose and unhealthy. The peritoneum was inflamed, and about from four to six ounces of serum were effused within its cavity. Upon again raising the uterus, the cervix was seen to be dark-coloured; and having divided it, the lower portion and orifice were found in a gangrenous state. The bladder was empty and uninjured. The brim of the pelvis was examined, and found fully as much distorted as I have before-mentioned.

Remarks.—The issue of this case presents indisputable evidence of the serious mischief arising from protracting the operation. Both the life of the mother and the child were most likely forfeited by the delay. How the real character of the case could have been so much overlooked, after the clear and decided opinion of Mr Wood given at the end of the seventh month, I am at a loss to conceive.

The tumefaction of the external genitals, and the inflamed condition of the vagina, are alone to be attributed to the too frequent examinations made. When a practitioner undertakes to explore for the exact measurements of the brim of a pelvis, deformed like the one belonging to the subject of this case, he is compelled to pass his hand completely into the vagina; and, from an anxiety to accomplish this, and to ascertain the nature of the presentation of the child, he is induced to repeat the operation very often. These repeated examinations are often productive of very serious mischief, causing inflammation, which frequently terminates in suppuration and sloughing. With these circumstances before us, we are of opinion that every unnecessary manœuvre ought to be avoided, and that the practitioner should acquaint himself, as completely as it is possible, with the nature of the case, before he withdraws his hand.

Case II.—Mary Moxon, aged 39, had been married sixteen years, and had been pregnant eight times, in seven of which she reached the full period of gestation, and in one she miscarried once at the fourth month, which happened about thirteen months before her present pregnancy. The last natural labour took place about four years since, and was so rapid, as to be completed in two to three hours. She had enjoyed good health until about two years ago, when she began to suffer from what she called rheumatisms and a short cough. Afterwards she was frequently confined to bed; and her friends observed her to rather diminish in height. The pains in her back and hips increased in violence during her present pregnancy, and her height is now very considerably diminished. She has been employed as an "ender and mender" for the manufacturers, which occupation has obliged her to be sedentary, but has attended to her domestic duties, although unfit.

At one o'clock on Thursday morning, May 24th 1821, she was apprised of the approach of labour by a discharge of water, which continued to dribble away without pain. At four o'clock, Mrs Barber, her midwife, was sent for, as she now felt slight pains. On examination *per vaginam*, Mrs B. could neither feel the *os uteri* nor any part of the child, but ascertained that the pelvis was considerably distorted. At noon she sent for Mr Wilson,* one of the surgeons of the Manchester Lying-in Hospital, but he was from home; and instead of immediately applying elsewhere, she allowed several hours to elapse before she sent for other assistance. At eight o'clock P. M. Mr K. Wood saw the patient, and considered the case of such importance as to induce him to call upon Mr Wilson, who was then at home, and they immediately went to the house, and reached it at a quarter before nine o'clock. Mr Wilson agreed in his opinion; and he desired that a general consultation of the medical officers of the institution might be immediately called.

At ten o'clock P. M., when Dr Hull, Mr Wilson, Mr K. Wood, Mr Lowe, and Dr Radford had assembled, the state of the patient was as follows. Her pulse was 130; the skin hotter than natural; her tongue was furred; she was very thirsty; her countenance was cheerful. She had passed urine at several intervals during her labour, and her bowels had responded three times to an enema which her midwife had very judiciously administered. The stools were scanty and of a green colour. The pains, which were reported to have been very frequent during the afternoon, continued so. She complained of great tenderness in the belly, which was considerably increased by pressure. The distance between the pubes and sternum was much shorter than natural. The *fundus uteri* projected very much forwards, and had an inclination to the left side. By an examination *per vaginam*, I found the parts soft, moist, and cool. The sacrum was considerably more incurvated than natural, and the coccyx projected upwards into the cavity of the pelvis. The tuberosities of the ischia approached very near together at the fore part; and the rami of the ischia and pubes approximated so closely together, as not to admit a finger to pass between them along any part, except at the middle, at which place there was a small opening, in consequence of a slight bulging outwards of the bone on either side. The pubic arch was destroyed, and only a small chink left, by which the depth of the pelvis was increased at the anterior part. In order to examine the brim, I passed my hand, but was compelled to carry it very much backwards. The pubes on each side formed a very acute angle at their fore part, and then running forwards nearly parallel to the symphysis, having a slit between them which would barely admit the finger edgeways. The base of the sacrum and the last lumbar vertebra had sunk forwards and downwards into the pelvis, and diminished the conjugate diameter on the left side so much, as barely to admit the finger in the position, that when it was withdrawn, it measured three-quarters of an inch, and when placed in other parts of the brim, an inch, as far as could be ascertained, was the fullest latitude which could be given to guide us in our decision. We all agreed that no other means but the Cæsarean section could avail us to deliver this poor creature.

---

* It is with the permission of my respected friend, Mr Wilson, that I am enabled to publish this case; and I beg to thank him for his great kindness, more especially for his liberality in furnishing me with what few notes he had taken of the case, all of which are in accordance with my own.

Having decided upon our plan, we stated our opinions to the friends, who readily consented that we should adopt any practice we thought best. When we acquainted the patient with the difficulty of her case, and the operation necessary to extricate the child, she unhesitatingly acquiesced. It was intended to have used the catheter, but this was unnecessary, as half a pint of urine was discharged by her own efforts.

She was now placed upon a table, and a little brandy and water, with thirty drops of laudanum, was administered her.

Mr Wilson made a longitudinal incision a little to the left of the umbilicus, six inches in length, extending from three inches above to three inches below this part, and divided the abdominal parietes down to the peritoneum. A small opening was made through this membrane, and it was fully divided by a probe-pointed bistoury, passed along with the finger. An incision was now made nearly through the uterus, corresponding in length and direction to the external wound. The probe-pointed bistoury was introduced on the finger through a small opening, and the remaining portion divided. This exposed the child, which lay with its breech towards the opening. Mr K. Wood seized the child by one thigh, and the body was extracted with the greatest ease, until the shoulders came to pass, when the uterus suddenly and powerfully contracted, and grasped the child's neck, and left arm so strongly, that this gentleman could not liberate it, although he used great force in extraction. He then gradually passed his hand along the body of the child into the uterus, and having dilated the structure, the child was extracted. It would have been easier to have torn away the uterus from its connections, than to have brought the child away by direct extractive force. The fundus and body of the uterus felt very hard. The child was vigorously alive when first taken hold of, but, from the length of time occupied in extracting the head, it became so enfeebled as to show only slight signs of life. I very diligently employed every means to resuscitate it, and continued them for at least three-quarters of an hour, but was ultimately unsuccessful. This was a most appalling affair. After dividing the funis, the placental extremity was firmly held with one hand, whilst the other was introduced into the cavity of the uterus, for the purpose of removing the placenta, which was already detached, and lying loose. The uterus then immediately fully contracted.

The intestines, which appeared at the wound, were replaced and retained by the extended hand; the edges of the wound were then brought together by ligatures, supported by slips of adhesive plaster and an extended bandage.

Very little blood was lost during the operation, a small branch of the epigastric only being divided. Its bleeding was restrained by the pressure of the finger.

The patient felt faint whilst on the table, but was soon recruited by taking a little brandy and water. When all was adjusted, she was carried to bed, and said she was quite as comfortable as she could possibly expect. The pulse now beat about 136 in the minute, and was distinct. The heat of the skin was not much above natural. In half-an-hour afterwards she felt a distressing sensation at the chest; her heart beat very quickly, and the breathing became very much hurried; her skin grew cold, and the vaginal discharge was increased, but still not in such quantity as to create alarm. Thirty drops of laudanum in a little brandy and water were immediately administered, and in half-an-hour forty drops more. In a very short time all these symptoms subsided, and she felt as well and as warm as before. All stimulants were now prohibited, and the antiphlogistic regimen recommended, and she was left for the night in the charge of Mr Hunt, at that time a pupil of Mr Wilson's.

Friday, May 25, 7 o'clock A. M. Present, Dr Hull, Mr Wilson, Mr K. Wood, and Dr Radford. She experienced no further palpitation of the heart; slept tolerably well; the pulse was 131; respiration easy; skin rather hot; belly feels comfortable and not swelled. To take a saline effervescent draught every three hours, and an ounce of the almond mixture with five drops of laudanum in the intervals.

12 o'clock noon.—Present, Mr Wilson, Mr Hudson, Mr K. Wood, and Dr Radford. The heart appeared to jerk; pulse

130, and quite distinct; skin hotter; her countenance more anxious; tongue furred, but moist; has again slept; urine passed twice. The medicines were continued.

4 o'clock P. M. Present, Mr Wood, Mr Wilson, Mr K. Wood, and Dr Radford. Belly rather tense; pulse 130, and firmer; tongue dry and furred; is thirsty; has passed urine; her cough is still troublesome; the bowels are constipated. A solution of Epsom salts in infusion of roses was directed to be given, until it operated, and a linctus for the cough was ordered.

6 o'clock P. M. Present, Dr Hull, Mr Wilson, and Dr Radford. Her countenance looked better; the heart throbbed violently; her pulse beat 125; the tongue was rather more moist and soft; the belly continued very tense; and the respiration was hurried; she has had slight vomiting, and her bowels have not yet been moved. The medicines were continued.

10 o'clock P. M. Present, Mr Wilson, Mr Lowe, and Dr Radford. Her countenance has become more anxious; her respiration is more laborious, and she has again vomited. The skin is hotter; her belly is very tender, and is much swelled; her pulse is 130; the vaginal discharge trifling, and very slightly coloured; she has a tendency to dose; has had several fœtid liquid stools.

To omit the aperient, but to continue the other medicine; the bandage to be loosened.

26th, Saturday, 8 o'clock A. M. Present, Mr Wood, Mr Wilson, Mr K. Wood, and Dr Radford. Has frequently vomited a brown slimy fluid; here pulse is 130; her respiration still laborious; the belly rather softer; her skin is still hot; and the lochial discharge very trifling.

10 o'clock A. M. Present, Dr Hull, Mr Wilson, and Dr Radford. Pulse 134; vomiting has ceased.

1 o'clock P. M. Present, Mr Wood, Mr Wilson, and Dr Radford. Her hands feel cold; the pulse is 130; her mind is clear; the vomiting has ceased; lochial discharge fetid and more profuse; and there has been a thin and offensive sanious discharge from the wound.

5 o'clock P. M. Symptoms still grow worse.

10 o'clock P. M. Mr Wood, Mr K. Wood, and Dr Radford. The symptoms continue to become more unfavourable.

Sunday, 8 o'clock A. M. Pulse 140, and very weak; the skin is rather cold and covered with a slight clammy sweat; her countenance is very anxious; and there is great swelling and tenderness of the belly; the lochial discharge is very offensive; she has not passed urine. The lowest strap of adhesive plaster being removed, the wound appeared in an unhealthy state and not united; a great discharge took place, which was very offensive. During this day (Sunday) she was visited several times, and found still further sinking. At six o'clock in the evening she expired, having lived sixty-seven hours and a-half after the operation.

Remarks.—It may appear strange that no notice was taken of this poor woman's case at an earlier period of pregnancy, as she was a patient of the Lying-in-Hospital. But our hospital extends its aid only to poor women at their own houses; and this poor creature having obtained a note of recommendation from a subscriber, was admitted, the medical officers having no knowledge that such a case was on the books. Another unfortunate circumstance was the midwife omitting to send for other surgical assistance in the absence of Mr Wilson, thereby allowing several valuable hours to elapse.

The violent contraction of the uterus, by which the head and left arm of the child were seized after the extraction of the trunk and lower extremities, forms a remarkable feature in the case. The placenta was found detached and lying loose in the cavity of the uterus, and how far this violent contraction depended upon this circumstance is difficult to say. In natural labour, we well know that, as soon as the placenta is detached, the energies of the fundus and body of the uterus, are aroused, and contraction follows, and is continued until this mass is expelled. In the former case of Cæsarean operation, the uterus was quiescent, until the placenta was detached by the hand, when contraction instantaneously followed.

To these two cases, I purpose to add some observations on the Cæsarean operation in a future number of this Journal.

Manchester, November 1840.

Art. VII.—*A Description of a Double Monocephalic Human Monster, which was transmitted to this country from South America, by* Robert Mackay, Esq. British Consul at Maracaylis, Venezuela. By Eric Mackay, M. D., Birmingham.

The subject of this paper was born at Maracaylis, in the republic of Venezuela, South America, in the month of March 1840.

It is the offspring of a negress, and is reported to have been born alive, but to have died a short time after its birth. Such are all the particulars which accompanied this singular monstrosity to this country. It is of the female sex, and seems to be composed of two equally developed bodies, united together from the umbilicus upwards, bearing four arms and four legs, and having only one neck and head common to both.

Judging from its size and proportions, it must have arrived at about the seventh month of uterine life, or nearly so. Its head is thickly covered with hair, the eyebrows are distinct, the bones of the head are firm, and the nails of the hands and toes are well developed. With the exception of a fissure extending from the right angle of the mouth to the right nostril, the features are perfectly formed and symmetrical. Indeed, if it be placed upon the table, resting upon its dorsum, and the head, neck, and shoulders are alone regarded, its anomalous structure would not be suspected, so closely do these parts approximate their natural form and appearance. The head exhibits an extraordinary breadth posteriorly, and is somewhat triangular in form, but the variety is not greater than occasionally is the result of difficult parturition. A nodule of integuments, inclosing a small quantity of adipose tissue, projected from the posterior part of the head. The face holds the same relation to the united trunks as it ordinarily does to the body; the spines are placed right and left, and the limbs attached to each follow a corresponding direction.

Previous to describing the anomalies observed in the soft parts, a few peculiarities in the skeleton will be specified.

The anterior and lateral parts of the scull were composed of the usual bones, naturally articulated with each other, but posteriorly two additional bones, of a triangular form, were discovered. They lay between the occipital and the parietal and temporal bones; their apices were situated a little below the lambdoidal suture, and their bases separated the occipital from the temporal bones at the base of the scull for about an inch and a-half, and were connected with each by membranous sutures. The perpendicular ridge of the occipital bone, instead of terminating or bifurcating where the *foramen magnum* commences, was continued into the basilar process, which was firmly connected to the sphenoid bone. At each side of this process, an oval orifice, about one inch and a half in its diameter, allowed the ascent of each spinal marrow within the scull. These orifices were partly bounded by the occipital and partly by the supernumerary bones just described, and upon their margins were two articulating surfaces, the one placed internally upon the occipital, the other externally upon the supernumerary bone, which corresponded with similar processes upon the atlas or first cervical vertebra.

The odontoid process of the second vertebra was absent. The spinal columns, almost in contact at their junction with the scull, diverged towards their inferior portion. The nasal, the palatal, and the ethmoid bones varied to such a degree from their usual form and arrangement, as to render an intelligible description of their size, processes, and disposition, exceedingly difficult, if not altogether impossible. The deviation from their natural relations existed chiefly upon their right side, corresponding with the fissure in the right nostril, but also extended to the opposite side.

Such is an enumeration of all the anomalies observed about the scull, comprising nearly all what has to be remarked with reference to the skeleton. The only other peculiarities worthy of being commented upon are the extraordinary expansion and curvature of the scapular extremities of the clavicles, in other respects well developed; and the great obliquity of the course of the osseous portions of the ribs, which almost touched each other, and whose cartilages ascended abruptly to their sternal attachments. The sterna were very partially ossified, but their size was of the normal proportion. They formed the principal connection between the two bodies; the anterior sternum, in relation to the face, articulating at its opposite margins with the anterior clavicles and ribs, the posterior sternum, by a similar arrangement, connecting the corresponding clavicles and ribs of both trunks. Upon examining the mouth a narrow attachment was perceived between the posterior portion of the tongue and the usual attachment of the middle part of the pendulous palate, which was wholly absent. This attachment being denuded, the pharynx was found to be well formed and rather capacious. At the lower part of the pharynx there were three orifices, the most anterior of which scarcely admitted a common probe, and after a short space divided into two impervious cords, which prolonged themselves into the right and left lungs. The second orifice, immediately behind the preceding, was about the size of a crow-quill, and proved to be the œsophagus; the third, directly posterior to the latter, and equally large, descended behind the œsophagus until it reached a third lung, lying in the posterior part of the common thorax, in which it divided and subdivided until it was lost in the tissue of the organ. The last mentioned opening had a thick and fibro-cartilaginous margin, somewhat resembling the *rima glottidis;* the superior portion of the corresponding canal likewise appeared to be a rudimentary trachea, being indistinctly divided into rings, but the canal previously described as commencing in the most anterior of the three orifices, and communicating with the lungs, did not present the faintest resemblance to either a larynx or trachea in any part of its course. Not a vestige of an epiglottis or of a thyroid gland was perceptible.

In the right side of the chest, and enclosed by the usual duplicatures of the pleura lay the right lung; in the opposite pleural cavity, and enveloped in the same manner, was the left lung. Both lungs were divided into the ordinary number of lobes. The third lung, situated immediately posterior to the pericardium, was unilobed, and about the size of either of the others. Its tissue seemed more developed than that of either the right or left lung, as we have previously stated, the canal, by which it communicated with the pharynx, evidently was so.

It has been already observed, that, in relation to the face, the sterna of the united trunks were disposed anteriorly and posteriorly, thus constituting opposite boundaries of the thoracic cavity. Laterally it was bounded by the vertebral columns, and by the ribs, which, by the peculiar arrangement previously described, completed the walls of the chest. The pleura lined the interior of the walls without much deviation from its natural arrangement; a tolerably large thymus gland occupied its usual position immediately behind the anterior sternum; the pericardium and its contents lay between the folds of the serous membrane, and, proceeding backwards, the pleura was reflected upon the posterior sternum, upon which lay the third lung, with those organs which usually occupy the posterior mediastinum.

The heart consisted of three cavities only, one large auricle occupying its superior portion, and two ventricles communicating with it by large orifices, guarded by the usual valves. At the right margin of the auricle, the cavæ entered, carrying the blood from the venous systems of both bodies. To the left, and posteriorly, the pulmonary veins entered the auricle, which was at this part concealed by the pulmonary artery and aorta. It was found impossible to follow the former vessel, so as to describe its course more minutely, without sacrificing more important organs. The aorta arose from the left ventricle by a single trunk, which after ascending a short space bifurcated, one division proceeding towards the right, the other to the left. Each of these divisions, where they came in contact with the corresponding spinal column, divided into three branches, of which one descended along the spine to supply the inferior portions of each body; another proceeded to the anterior superior extremities, and the third ascended along the neck to the head; by this arrangement supplying the head by means of two principal vessels, which observed the same course and relations as the carotid arteries do in ordinary cases. From the arch of the descending branches vessels proceeded, which took the course of the subclavian arteries, and thus were distributed to the posterior superior extremities. The descending branches then proceeded along the spines, giving off minor branches in all directions, similar to the descending aorta, and like it also, each sent two hypogastric branches to accompany the single umbilical vein.

The diaphragm separated the large thorax from the abdomen—was firmly attached to the internal surface of the posterior mediastinum, accommodated itself to the unusual form of the lateral walls; but in other respects held the same relations as in the normal condition. That part of the abdomen which is above the umbilicus was a single cavity, common to the united bodies. Below this point it was in all respects double, and each division had its distinct parietes, with the exception of the peritoneal lining, which was continuous throughout. The stomach occupied nearly the centre of the abdomen, and from it proceeded the intestinal canal connected by the folds of the peritoneum to both abdominal cavities. At first single, it was afterwards bifurcated, and each division attached itself to, and continued its course, in one of the abdominal cavities, until its termination in the anus. The length of the canal to the point at which it was divided was three feet; and it is rather singular that from this point, which was central in relation to both cavities, that which proceeded to the right side was only eighteen inches long, while the other division proceeding to the left measured two feet. No peculiarity worthy of particular notice was observed in those organs which usually lie in the superior half of the abdomen; they were single, and not larger in proportion than those which occupied its lower half, which were double.

The urinary and genital organs were double, well developed, and placed in their natural positions. The lower portion of the colon and rectum were filled with a greenish substance, resembling meconium.

As might be anticipated from the description which has been given of the structure of this singular monster, the spinal and ganglionic systems of nerves were double, while the cerebral was single. The distribution of the spinal and ganglionic nerves was examined with some care; but no important deviation from their ordinary courses was observed. The spinal columns from each vertebral canal entered the scull by the openings at the sides of the basilar process which have been described. They united immediately as they entered, and continuous with them was a mass of cerebral matter about the size of a hen's egg, exhibiting no trace of division into lobes or hemispheres, from which the usual number of nerves proceeded. The spinal columns presented the same appearance within the scull as they presented in the vertebral canals, so that from this circumstance, and the nerves proceeding from the *medulla oblongata* being single, it may be inferred that that portion of the cerebral mass was likewise single, although it could not be distinguished from the rest of the imperfectly formed brain.

From the preceding account, it will be perceived that in some respects this monstrosity presented the characters of a single body; in others of two distinct bodies. Thus the spinal and ganglionic nervous systems were double, while the cerebral was single, each corresponding with the organs which they were destined to supply. The organs of mastication and of deglutition were single, as were also those of digestion, with the exception of the inferior portion of the alimentary canal, which at a certain point became double. The respiratory organs were the least developed, and the most anomalous in their character, there being two lungs situated in their usual position, and a third lying upon the posterior sternum, which seemed to belong equally to both bodies. With the exception of the heart and the vessels which supplied the head, neck, and such organs as were single, there were two complete arterial and venous systems, which, with insignificant varieties, were disposed in the usual manner.

The urinary and genital organs were double, and well developed.

ART. VIII.—*Malta, considered with reference to its eligibility as a place of Residence for Invalids.* By FRANCIS SANKEY, M. D. Universitatis Melitensis.

THE object of the following pages is to direct the attention of medical men, in England and elsewhere, to the advantages possessed by this island, as a place of temporary residence for such persons as require a change of climate for the restoration of their health. In speaking of Malta as a resort for the delicate in health, I, of course, must be understood to mean during the winter season only, or from the beginning of October to the end of May. I state this at once, as it seems to me to remove every objection offered by writers on the climate of Malta, who talk of the oppressive heat, and depressing winds, of the four summer months.

No medical man, in the present day, would think of recommending his debilitated patient to try the relaxing influence of a southern climate during the summer.

My intention is, to state with candour and brevity, what Malta has to offer to any one who may visit it in search of health; for a delusion has very generally prevailed concerning Malta in a hygienic point of view. People are impressed with the idea that the heat here is alarmingly oppressive;—that certain winds are often harassing the constitution with their debilitating influence;—that few conveniences can be obtained for securing domestic comfort; and that it is an arid rock, where a garrison of soldiers, living in a fortress, are supplied only with their rations of beef and rum; in fact, that Byron's description of the place is a faithful picture. Byron evidently composed his lines under the influence of spleen and misanthropy; besides, the state of Malta, in the present day, is very different from what it was some thirty years ago. It is no longer a " military hot-house." The course of the last few years has brought about a rapid amelioration in the island, whereby the mode of life is almost entirely assimilated to that in England and France.

The island lies in about the 36th degree north latitude, south of Sicily, and nearly in the centre of the Mediterranean. It rises prominently above the level of the sea, to an altitude of something more than 600 feet at its highest elevation. It presents an oblong appearance of about eighteen miles long and twelve broad. Many of its valleys and ravines are exceedingly fertile; but much of its area is barren and uncultivated.

The surface of the island is undulating. The cultivated portion is divided into terraces, and small sections, by low stone walls, concealing the richest verdure within, but presenting to the eye, when viewed from a distance, a look of utter barrenness. Woods there are none, nor any collection of trees deserving the name of a grove. The Carouba, a low, spreading, and deep-coloured evergreen tree, is scattered plentifully along the sides of the hills.

The orange and the lemon plantations are enclosed within garden walls, and form no feature in Malta scenery.

To the west of Malta is another and smaller island called Gozo; and in the intermediate strait is the small islet of Comino.

The physical characteristics of all these islands are perfectly analogous. Their population amounts to about 120,000, including the military residing in villages called Casals, scattered over the islands, and in the city of Valetta, with its populous suburbs. The city of Valetta may vie with any town on the shores of the Mediterranean, for the elegance of its construction. It is built upon a small peninsula, which is situated between two arms of the sea running nearly parallel into the land, forming two magnificent harbours. The streets are broad, and intersect each other at right angles, thus dividing the houses into large quadrangular groups. The houses are solidly built of stone, of two, or at most three, stories high. The roofs being flat, and guarded by low parapets, afford agreeable promenades, whence, for the most part, you may obtain a view of the sea and of the country. The rooms are large and lofty, and are generally furnished with fire-places, a comfort introduced by the English since the island has become a British possession. The covered projecting balconies, in front of the houses, have a picturesque and striking appearance.

The streets are paved or macadamized: they are quickly dried after rain, and are kept exceedingly clean, so that nothing is suffered to remain which might produce noisome effluvia or engender disease. The inns are numerous and good. There is no longer any difficulty of obtaining commodious apartments or furnished houses; and living is much cheaper than in England.

Residences in the country, or on the sea side, with gardens attached, are to be had at short distances from the city.

The markets are well supplied with meat, poultry, vegetables, and fruit.

There is very little game; it principally consists of those migrating birds, which, in their transit to and from Europe and Africa, alight on this island. These are chiefly quail, plover, and beccafighi.

There is a plentiful supply of fish, little of which would be considered by the ichthyophagic epicure as really good.

No place can boast of a greater equality of temperature. Throughout the winter, an entire day without sunshine, or an entire day of rain, is equally rare, although it does occasionally occur.

The thermometer does not vary more than four or five degrees during the twenty-four hours. From the end of September to the end of May, the temperature is most delightful; a complete spring reigns in the island. Even in January and February, the temperature is never so low as to be disagreeable to persons coming from a more northern clime.

The following may be considered as a tolerably fair average of the thermometer during the year, taken daily at the hours of nine, twelve, and three, during each month of the year 1838.

The eight temperate or winter months.

	Max.	Med.	Min.
October, .	70	69½	69
November, .	65	64	63
December, .	58	56½	55
January, .	56	53½	51
February, .	58	55½	53
March, .	59	57½	56
April, .	62	60½	59
May, .	71	70	69

The four summer months.

	Max.	Med.	Min.
June, .	75	74	73
July, .	82	79½	77
August, .	82	80	78
September, .	77	76½	76

In February and March, there are severe gusts of wind, which sweep over the island. In the city of Valetta, from the rectangular position of the streets, the wind gains increased force; but although violent, it is not like the keen and cutting breeze which descends from a snow-covered mountain range. The heat in summer is moderated by cooling currents of air, unobstructed by hill or forest. In this respect, Malta has decidedly the advantage over Italy and the south of France, where the changes of temperature, according to the direction of the wind, are as violent as they are sudden. The much talked of and dreaded scirocco is by no means so terrible as has been represented. On this head, much exaggeration has gone forth. Dr Hennen, in his medical topography, and others, from Brydone to the present day, have written strange absurdities regarding this wind. The effects attributed to it have been ridiculously magnified. To it the fanciful ascribe all their morbid feelings. The imprudent, eager to palliate their indiscretions, find a ready excuse in the sciroc. At some seasons of the year it is certainly unrefreshing and disagreeable, more particularly so in the months of August and September. It blows from between the east and south, and is a hot and humid wind. The atmosphere is usually overcast with a hazy vapour, communicating to everything a moist and somewhat clammy feel.

In Africa it is an exceedingly dry and rather strong wind; but when it arrives at Malta, having passed over a considerable expanse of sea, it becomes saturated with aqueous vapour. The autumnal scirocco has, doubtless, a considerable influence over the animal system: it throws a degree of inertness over both the mind and the body. This kind of languor and lassitude is felt chiefly by those persons who are of a nervous temperament, of an indolent disposition, or by those who continue, in a southern latitude, the luxurious habits of a more northern climate; but this wind rarely blows for more than one day consecutively. In the winter, it is soft and balmy. Several invalids, with dry cough, and bronchial irritation, have expressed themselves as always feeling better during its continuance. Dr Hennen's account of Malta, in his medical topography of the Mediterranean, should be read at present with considerable limitations. His remarks are generally good, but he was certainly most extravagant when he wrote about the " implacable heat" and " showers of mud."

The range of the thermometer in the course of the year is from about 50° to 85°, and the medium temperature in the twelve months is never above 70°. For the " showers of mud," we must read dust, which, wafted before the wind, may in part be united with the aqueous vapour in the atmosphere, and thus be deposited over the land,—a thing which happens by every roadside in England.

But as we are not advocating a summer residence here to the debilitated invalid, it were vain to consider farther the errors and exaggerations of former writers. Indeed, the sources of objection either no longer exist, or their authors were misled by erroneous views.

In Dr Hennen's time there existed a slight source of malaria in the stagnant water situated at the head of the great harbour. This place, however, has been entirely drained, so that there is no longer any spot of marshy stagnation in the island to give origin to malignant fevers.

There are no vegetable matters running into decomposition, no animal substances allowed to putrify above ground. The island is open to every wind that blows, so that there is nothing in the natural state or geographical position of Malta that necessarily generates any kind of hurtful malady.

In the very able statistical reports by Major Tulloch, published in 1839, from official returns, some serious errors occur. The report states that Malta is not so healthy as Britain. This report is founded on the deaths occurring in an average population of 100,000 people compared with the same number in England. It is not just to ground such statement on the deaths alone. The proper basis of the calculation would be the number of the sick; for such is the difference between the two countries, that a simple affection, which, in England, would be cured in a few days, is allowed by the natives to degenerate into serious disease. The people cannot pay for good medical attendance, and are averse to taking any sort of medicine. They have not the means of changing a scanty and unwholesome diet for more nourishing aliment. The people in the country eat the coarsest description of food, bad bread, crude vegetables, olives, and inferior kinds of salt fish. Meat they seldom touch, and wine only on festival days; and then perhaps to excess. The long fasting of forty-four days in Lent, in addition to the generally unhealthy course of diet, is another source of debility, and therefore, a predisposing cause of disease. From these circumstances, coupled with bad clothing and dirty habits, the people here have not sufficient energy to support disease, so that of an equal number of patients in England and Malta, a far greater proportion would recover in the former country. If due allowance be made for all the diseases which the inhabitants bring on, or continue on themselves, either by carelessness or necessity, there will not be found in the south of Europe a more healthy spot than this island. The mortality here, notwithstanding the causes above stated, cannot, in ordinary years, be calculated at so much as 3 per cent. The average number of deaths annually, according to the Statistical Report, for thirteen years, is 2577, which, on an average population of 100,270, amounts to rather more than 2½ per cent.

The existence of the predisposing causes of want of energy, impure circulating fluids, want of the means of prevention and of cure, should tend to keep the population stationary, if not cause its number to retrograde. But the excess of births over deaths is very considerable. If, then, an annual augmentation can be shown, it must be conceded that the island is a salubrious place; and this fact must prove the fallacy of all that has been advanced to the contrary.

Another most erroneous statement exists in the table of returns for pectoral complaints. This error arises from a mistake in the meaning of the term consumption, as it is applied by the Maltese practitioners. These gentlemen understand by this term, any wasting of the body, from whatever cause it may arise, such as general scrofulous glandular disease, the natural decay of age, or a frame worn out by constitutional irritation, even if that irritation be produced from accidental violence.

In the surgical department of the hospitals, all cases of death which take place from purulent discharge or constitutional irritation, subsequent to the receipt of some severe injury, are returned by the surgeons of these establishments as cases of consumption, and registered as such in the police reports. Very recently, a respectable lady, the wife of a judge, died from the exhaustion consequent to a severe burn received some time previous to her decease. This lady's death was returned as a case of consumption. A woman died of abscess in the uterus, occurring after a laborious parturition. The cause of her death was returned under the same sweeping denomination. Consumption, as it is understood here, has not necessarily any reference to pectoral disease. In the case of a chronic *pulmonitis* terminating in death, where there are no tubercular deposits in the lungs, the disease may be called consumption; whereas, if these tubercles existed, the complaint would be called phthisis; so perfectly distinct are the limits of these affections in the mind of the native practitioner.

The following are the words of Dr Bardon, surgeon of the Civil Hospital, to an inquiry as to the meaning he attached to the term consumption. "By consumption is to be understood the wasting and emaciation of the body to the utmost degree. This effect, arising from a diminished power in the digestive action, may have many primary causes; any irritation, wheresoever may be its seat, or whatever may be its nature, can give origin to the state called consumption; for instance, neuralgic affections, violent and prolonged pain, continued mental emotion, and particularly nostalgia, may be followed by this wasting of the body termed consumption. The encephalon being disordered, will involve other viscera, and more especially enfeeble the digestive power. Wherever morbid action is established, consumption may follow as a consequence. It may be caused by acute, as well as by chronic disease, but more generally by the latter."

The term is so well understood here, that it is a matter of extreme surprise to me to find the person sending home these returns ignorant of the fact. Under this class, therefore, there must be assembled a vast number of diseases which are not pectoral, or if pectoral not phthisical; and this monstrous error has been adopted and continued in the official reports under the head of diseases of the lungs. There is an attempt to correct or palliate the mistake in a slight marginal note, but the calculation is made in the error, and is, therefore, necessarily false. According to the tables in that report for thirteen years, in an average population of 100,270, there died, by consumption and phthisis pulmonalis, 4149 or 319 annually. Of these cases, 2786 were not phthisical diseases, which number being deducted, leaves 1368 cases only of pulmonary consumption, or nearly 105 annually, amounting to about one in a thousand of the civil population. The whole number of diseases of the respiratory system of all kinds, for the same period of thirteen years, is returned 6664; from which, deducting diseases which ought not to be classed under this denomination, we have 3878 or 298 yearly, about three in a thousand of the whole population.

Major Tulloch, in his report, says, that a greater proportion of the British Troops die in Malta than in England; from which he concludes, that the climate is unfavourable to health. May we not rather look for a cause of this increase of mortality in the well known fact, that soldiers are generally addicted to strong drink? that the thirst produced by the heat prompts them to gratify the propensity to excess, which they can do so readily in consequence of the low price of wines and ardent spirits? To this vice we may fairly attribute the increase of mortality among the foreign troops on the Mediterranean stations. Let us compare this assertion with the report in the same work on the health of the Maltese Fencible regiment. We observe, that the average mortality in this corps is amazingly reduced by the superiority of their food over that of the other inhabitants, as well as by the greater attention given to their health, together with their habits of sobriety. The report says, "this corps was organized in the year 1825, and is composed of Maltese, enlisted for a limited period of service, with the understanding that they are not to be employed off the island."

By an extract from the War Office returns, it is shown, that for eleven years the strength of this corps averaged 515 men, of whom $4\frac{7}{11}$ have died annually, being at the rate of nine per thousand, which is less, by one-half, than the mortality among the foreign troops, all men of the same age, vigour of constitution, and placed under precisely similar circumstances. How shall we attempt to account for this marked difference? I think we may justly affirm, that it arises from the different habits of the men. It cannot be, as the author of the report would infer, that, as the Fencible soldier is indigenous to the climate, he enjoys greater immunity from disease: for if so, we should find, that the foreign families resident on the island, and those in the civil employ, would suffer in proportion to the troops of the line, which is not the case.

The author of the report goes on to say, that there are two circumstances, independent of climate, to which the exemption from disease, which these native soldiers enjoy, is mainly attributable. The comparative abstinence from those excesses, to which the British soldier is addicted, and the fact, that many of them being married men, are therefore likely to be free from those diseases which constitute so large a number of the admissions into our hospitals. Thus the reporter refutes, in a manner, the errors of his own statement, and proves the truth of the assertion, that the mortality among the foreigners is more dependent on their habits than on climate, and that the number of deaths among the general civil population, is owing more to their necessities than to any atmospheric or telluric influence.

I do not, of course, pretend to affirm, that the climate and air of Malta possess any curative virtue for those persons, whose organic diseases have placed them beyond the power of recovery elsewhere. Where tubercular disease has proceeded so far as to make extensive ravages in the pulmonary tissue, it is ever useless and cruel to send such unhappy persons far from their homes and sympathizing friends, to die among strangers in a foreign land.

At the present day it is pretty generally understood by medical practitioners, that such removal is mischievous, both from the fatigue attending the journey, and chiefly that the change to a warmer latitude, by increasing the irritability of the hectic sufferer, and augmenting the nocturnal perspirations, would farther reduce the strength of the patient, and hasten the catastrophe. Yet sometimes the medical attendant trusts that disease has not committed the ravages he suspects, or he is induced to yield to the wishes and solicitations of the patient, whose hope of recovery often appears commensurate with the fatal tendency of his complaint.

But to persons who are suffering from constitutional debility, who, either from conformation or accidental circumstances, are strongly predisposed to pulmonary disease, or are in any state of cachexia, unattended with considerable organic lesion, a residence in a southern latitude, for three or four months during the winter, is highly useful; and I think Malta offers advantages to such persons, equal to those of any other place. Among those advantages we may enumerate,—the voyage by sea, when land travelling would be too fatiguing; the facility of conveyance from any part of the surrounding continents; the novelty of the scene, so different from any thing which Europe can offer; the living under the British flag, in a British possession; the many comforts that may be commanded; the power of obtaining the assistance of English medical practitioners resident in the island, as well as those of the staff of the army, or navy; and a climate where, on every day of the year, for some hours of that day, exercise in the open air, or gentle boat exercise, may be taken, so essential to the re-establishment of health.

Another advantage which Malta possesses over almost every other place, is the great facility which the invalid has of getting away, should he find, or fancy he finds, the climate uncongenial to his constitution.

The almost daily arrival and departure of English, French, and Italian steamers, will enable him to try other countries, to make excursive trips, and again to return,—thus frequently changing the air and scene, both of acknowledged use to the infirm.

I have known persons, whose health has been considerably improved by residing for a while in Malta, and making short voyages to the neighbouring states.

The great benefit derived by numerous invalids, that for several winters past have visited Malta, ought to silence the objections of those whose opinions are founded on the erroneous data in the otherwise excellent report of Major Tulloch, or in the medical topography of Dr Hennen.

The residence of her Majesty the Queen Dowager Adelaide, during the winters of 1838–9, has tended greatly to give a deserved celebrity to this island: and her liberality has added a handsome church to its public buildings.

To the mere seeker after pleasure, it may be said to be wanting in matters of deep interest or excitement. There is little extent of country, and diversified scenery; no extensive lawns, nor woods, nor streams of water; and no great variety of rides or walks. Your exit from the city must be by the same fortified gate, over the same drawbridge. There is little room for the display of brilliant equipages; and society is not so artificial as in the great European capitals.

In the way of amusements, Malta can offer little beyond the following:—namely,

A theatre for Italian music, with very respectable vocal performers, and an excellent orchestra; public promenades, where the military regimental bands play at certain hours of the day; a garrison library, and a public government library, which are accessible to every one; a subscription club, with mercantile and subscription reading-rooms; numerous public balls during the winter; saddle horses, or close and open carriages, to be had at all times on the most reasonable terms; and yachts and numerous smaller pleasure boats for excursions round the island.

Valetta cannot be said to be a monotonous, or a dull place. The presence of the garrison and the fleet; the influx of strangers from all nations; the continual arrival and departure of ships of war and packets, keep the mind amused by the continually shifting scene, and render Malta equal to any place in liveliness of aspect.

In putting down the preceding remarks, intended to direct the attention of my professional brethren to Malta, as a desirable place of residence for the invalid during the winter, equal if not in most respects preferable, to any other place in the south of Europe, I may have been prompted by the laudable desire to favour this British possession; but I trust it has been done with a strict regard to justice, without either indulging in extravagant praise of this, or disparaging the merits of other places; a course of conduct which, I feel convinced, would injure my cause with every discerning person.

ART. IX.—*On the Diurnal Variations of the Pulse in Disease.* By WILLIAM AUGUSTUS GUY, M. B., Cantab. Professor of Forensic Medicine, King's College, London, and Assistant Physician to the King's College Hospital.

IN the eleventh volume of the Edinburgh Medical and Surgical Journal, Dr Knox published two essays on the diurnal variations of the pulse, which went far to overturn the old theories on this subject. The observations of Bryan Robinson * and Falconer, † though wanting in accuracy, had certainly justified the older physiologists in the view which they took, that the pulse is more frequent in the evening than in the morning. Dr Knox's observations made on his own person, and on one other healthy male, and supported by the evidence of one of his friends who had made some independent observations on his own pulse, seem to have left no doubt on his mind that the former theory was incorrect, and that the pulse becomes less frequent instead of more frequent as the day advances. Since, however, the observations of Robinson and Falconer, though in some respects badly planned, show a very considerable increase of frequency towards evening, the two series of observations reported by Dr Knox, though backed by a general verbal statement of the experience of a third party, cannot be held sufficient to overturn the old theory, and to establish a new one on its ruins. Accordingly, impressed with the necessity of collecting new facts in illustration of this interesting question, I made the series of observations detailed in the eighth number of the Guy's Hospital Reports. The result of these observations, made with all the care and minute accuracy which such researches demand, lent new confirmation to the opinions of Dr Knox; but still left the question undecided. It was obvious, indeed, that such a question could only be satisfactorily answered by extending these isolated series of observations made on one or two individuals, to a larger number of persons. This,

* Animal Economy, 1732, p. 148, 150.

† Observations respecting the pulse, intended to point out with greater certainty the indications which it signifies, especially in feverish complaints. By William Falconer, M. D. Physician to the General Hospital, Bath, 1796, p. 26.

as occasion offers, I yet hope to accomplish; but, in the meantime, I have gleaned in the course of my reading a number of facts, which tend to throw additional light on this question, and go far towards bringing it to a decision. Of these facts, some have already come before the public in the pages of the Medical Gazette, (June 1839,) and others have since fallen under my notice.

With one or two exceptions, the observations thus gleaned from authors were not made with a view of deciding the question of the diurnal variations of the pulse, but formed part of experimental inquiries into some other function of the human body, or into the effects of remedies upon it. This circumstance rather enhances than impairs the value of these facts, as it removes one objection which might be urged against them, viz. that they were made with a view of supporting some preconceived hypothesis.

Of these independent evidences, all of which are averages of a number of observations, there is only one which favours the old theory. This is an average of 256 observations made in the morning, and 275 in the evening, by Dr Keill* on his own pulse, and forming part of elaborate tables, in which the perspiration, the secretion of urine, and the pulse are noted for each day in the year. The observations made in the morning extend from 6 A. M. to 10 A. M.; those of the evening from 10 P. M. to 1 A. M. The average frequency in the morning is 80.50, that in the evening 90.72.

I have met with but one additional evidence in support of Dr Knox's view since the publication of my paper in the Medical Gazette. This has been obtained from Dr Saunders's work on Pulmonary Consumption, and is drawn from observations made on his own pulse, preparatory to experiments on the effects of digitalis. In the morning the pulse was 60, in the evening 56. One fact taken from Dr Prout's† experimental inquiry into the quantity of carbonic acid eliminated at different times in the day, leaves the question I am examining undecided, as the pulse has nearly the same frequency morning and evening.

The evidence hitherto collected as to the question of the frequency of the pulse morning and evening in healthy males may, therefore, be thus summed up. For the old theory the observations of Keill, Robinson, and Falconer; for Dr Knox's theory, his own observations on himself and a friend (Mr S.;) Nick's two series of observations on himself and another healthy male; Dr Saunders's observations on his own pulse; and my own experiments on myself. The facts gleaned from Dr Prout's essay leave the matter doubtful. If these authorities are allowed equal value, the facts in favour of Dr Knox's theory are to those in favour of the old theory as six to three, or two to one. But these numbers by no means represent the actual value of the evidence for and against Dr Knox's theory, as the observations of Knox, Nick, and myself are far more accurate than those of Robinson and Falconer, and therefore entitled to greater confidence; whilst the twofold observations of Nick [*] seem to explain, in a satisfactory manner, the opposite results to which the observations of Robinson and Falconer led them. The facts gleaned from Hohl[†] with regard to pregnant women, the new-born infant, and the fœtus *in utero*, lend strong confirmation to Dr Knox's theory, and, together with the facts already adduced in its favour, leave little doubt that it is at least the rule, however numerous the exceptions to it may be. I myself entertain no doubt that the theory of Dr Knox is the true one, though I am as firmly convinced that frequent exceptions to the rule will be encountered. The observations of Keill, Robinson, and Falconer, show an excess of the evening over the morning pulse[‡] too considerable to be entirely explained away by the absence of that care in planning their researches, which gives to observation the character and value of experiment; and I am strongly inclined to the opinion, that at least one of these results forms a real exception to the general rule, that the pulse is more frequent in the morning than in the evening.

---

* Medicina Statica Britannica, 1718.

† Thomson's Annals of Philosophy, Vol. ii. and iv. 1813–14.

No observations have yet been made on the diurnal revolutions of the female pulse, if we except the facts gleaned from Hohl, which give an excess of the morning over the evening pulse of 2.40 beats. This result is obtained from twenty-five observations made in the morning and twenty-five in the evening, on the same number of pregnant women, one series of observations being made on each woman.

The diurnal variations of the pulse, when under the influence of remedies, is attributed by a single fact recorded by Dr Saunders in the work already quoted. During five days that he took from fifteen to twenty-five drops of the tincture of digitalis twice a-day, the pulse was uniformly more frequent in the morning than in the evening; the numbers being on the first three days 70 in the morning and 66 in the evening; and on the next two days, 76 in the morning, and 70 in the evening. On the seventh and eighth days, on each of which he took twenty-five drops of the tincture twice a-day, the pulse rose to 80 in the morning and 90 in the evening.

---

[*] See Medical Gazette, June 1839.

[†] Die Géburtshülfliche Exploration. Bey Anton Friedrich Hohl.

[‡] Bryan Robinson's experiment gives 68.50 as the mean pulse in the morning, and 78 as the mean pulse in the evening; those of Falconer give 69.6 in the morning, and 76 in the evening; and those of Keill 80.50 in the morning, and 89.72 in the evening.

The foregoing observations on the diurnal variations of the pulse in health, and under the operation of remedies, seemed to form a necessary introduction to the inquiry into which I am now about to enter. As I have already more than once had occasion to state,[*] it has hitherto been always assumed that the pulse in disease follows the rate of the pulse in health, according to the old theory, and is much more frequent in the evening than in the morning. Fordyce[†] is the only author who has thought it worth while to test the correctness of this assumption by actual observation, and he is opposed to the prevalent belief. He says, " In rheumatic and other inflammatory fevers, I have examined the pulse by the watch two or three times a-day, for seven, fourteen, seventeen, twenty-one, or even more days together, without finding it vary three strokes in a minute—not even at noon and midnight, when sensible alterations are said to be observable in the pulse." This observation of Fordyce, deprived as it is of much of its value, by being couched in general terms, is the only one which I have hitherto met with bearing on the question of the diurnal variations of the pulse in disease.

During the months of August and September last, I had an opportunity of making some observations on the diurnal variations of the pulse in the wards of the King's College Hospital. I made my visits twice a day, one at 9 A. M., and the other between 7 and 8 P. M. These hours were nearly equally removed from the breakfast and supper of the patients, and from the taking of the first and last doses of their medicines ; so that, as far as possible, the subjects of my observations were placed in the same circumstances at the two hours of the day. Whenever any difference existed in the remedies administered at the two periods, the observations were omitted. All the observations were made in the recumbent posture. As the number of patients at that time in the hospital was not considerable, I was prevented from multiplying my observations as much as I could wish ; but, as far as they go, they may be depended upon, as they were made with great care and due eaution. [‡]

The observations made on males are arranged in Table I.; those made on females in Table II. Each table contains the maxima, minima, and mean frequencies, morning and evening, arranged in such a manner as to admit of easy comparison. I shall state very briefly the results of these observations, in the two sexes respectively, and first in males.

---

[*] Guy's Hospital Reports, No. viii. and Medical Gazette, June 1839.

[†] New Inquiry, p. 53, 54.

[‡] I must here acknowledge my obligations to Mr Storks, at that time acting as clinical clerk to Dr Budd, for his kind assistance, and for more than once collecting the observations in the evening, when I was prevented from attending at the hospital.

The average of all the maxima, minima, and mean frequencies respectively are as follows :—

Morning.			Evening.		
Maximum.	Minimum.	Mean.	Maximum.	Minimum.	Mean.
90	74	82	89	71	79

Hence the average frequency of the pulse in the morning exceeds that in the evening by from one to three beats. An average of 109 observations in the morning, and 104 in the evening, being all the observations from which the tables are formed, gives 82 in the morning and 81 in the evening, a difference of only one beat. On comparing all the observations made in the morning with the observations made in the evening of the same days, it appears, that, out of 104 pairs of observations, there are 59 in which the pulse is more frequent in the morning than in the evening, 36 in which the reverse obtains, and 9 in which the pulse has the same frequency morning and evening. Hence the rule is, that the male pulse in disease is more frequent in the morning than in the evening, but the exceptions are more than one in every three.

On inspecting the averages in the table, it will be seen, that, in the case of the maxima, there are five instances in which the pulse is more frequent in the evening than in the morning, and three in which the frequency is the same at both times of the day; in the case of the minima and mean frequencies, the exceptions to the rule are also five in number, and one in which the frequency is the same morning and evening. The averages, therefore, give about 6 exceptions in 18, or 1 in 3, and therefore correspond closely with the observations taken singly.

The 16 first averages, from four groups, which may be conveniently contrasted, as in the following table :—

	Morning.			Evening.		
	Max.	Min.	Mean.	Max.	Min.	Mean.
Phthisis, four cases, -	104	87	96	105	82	92
Bright's disease, three cases, -	87	72	78	87	68	75
Rheumatic affections, four cases, -	81	71	77	85	70	77
Affections of the nervous system, five cases,	90	88	81	83	68	75

With the exception, then, of the rheumatic affections, these averages confirm the rule already laid down, in a very remarkable degree, in the case of the affections of the nervous system, less decidedly in the case of *phthisis pulmonalis*.

It is worthy of remark, that *phthisis pulmonalis* is the disease in which the pulse was formerly supposed to be very considerably increased in frequency towards evening; and Dr Beddoes, in his work on consumption, distinctly states such an increase to take place. So far from this being the rule, however, it appears that the three first cases contained in the table give a decided opposite result, as does also the case of phthisis contained in the table of female pulses. The fourth case in the first table forms an exception to the rule, and confirms the old theory; but even this case resembled the other cases contained in the table during the first days of duration, and the pulse only increased in frequency towards evening as the disease was drawing near to its fatal termination. Thus the mean of the first nine observations gives 100 for the morning pulse, and 95 for the evening, being an excess of five beats in favour of the former. It is the last five observations only which show an uniform increase towards evening, and that to a very considerable extent; the mean frequency in the morning being 92, and that in the evening 117, showing an excess in favour of the evening pulse of 25 beats. In two instances the excess of the evening over the morning pulse exceeded 40 beats, the number in the morning being 84 and 76, in the evening 128 and 122. If these very remarkable exceptions to the rule are excluded from the tables, the pulse in phthisis would show a much more considerable decrease towards evening.

I now proceed to examine the female pulse in disease. The results are here the exact reverse of those obtained in the case of the male pulse.

Seventy-seven observations made in the morning and 76 in the evening give the following mean result. Morning 85, evening 88, difference 3.

The table itself gives the following averages of the several maximum, minimum, and mean frequencies. Morning, maximum 91, minimum 75, mean 82; evening, maximum 98, minimum 78, mean 86, being an increase in the evening of from 3 to 7 beats.

On comparing all the observations made in the morning with those made in the evening of the same days, it appears that of 76 pairs of observations, there are 51 in which the pulse is more frequent in the evening than in the morning, 25 in which it is less frequent in the evening, and 4 in which the frequency is the same at the two periods of the day.

Hence to the rule that the female pulse in disease is more frequent in the evening than in the morning, there is, as nearly as possible, one exception in every three.

On examining the averages in the table, we find that in the maxima there are seven exceptions to the rule, and one instance in which the pulse has the same frequency morning and evening; in the minima and means respectively, there are five exceptions, and one instance in which the pulse has the same frequency morning and evening. Hence the exceptions in the averages, like those to the individual observations, are nearly as 1 in every 3. There are none of the observations on the female pulse, if we except the last five in the table, which admit of being thrown into groups;

and I regret that the cases in the hospital at the time of making my observations were not such as to admit of a more exact comparison between the male and female pulse in disease. The facts, indeed, which I have collected are not sufficiently numerous to admit of any minute comparisons, but merely to show the diurnal variations of the pulse in disease, using that term in its most general sense. The subject appears to me to be one of sufficient interest to deserve a more minute examination at the hands of those who have leisure for investigations of a scientific rather than a practical character; and as it is one of those subjects which may be successfully followed out by the younger members of the profession, provided they exercise but an ordinary degree of care and circumspection, it is to be hoped that it will receive still farther extension, and be carried into more minute detail.

The facts established by my observations are the following :—

1. The pulse of males in disease, like that of healthy males, is *more* frequent in the morning than at night; but to this rule there is one exception in every three observations.

2. The pulse of females in disease is *less* frequent in the morning than at night, and to this rule, likewise, the exceptions are one in three.

This remarkable difference in the pulse of the two sexes is in strict keeping with observations made in a state of health, and I have no doubt that the more minutely the subject of the pulse is examined, the more strikingly will the male and female pulse be found to differ. I have already pointed out some remarkable differences which ought to be borne in mind by the physiologist as well as by the practical physician.

I know of no very important practical results to which these observations tend. The differences are so irregular, and the exceptions to the rule so numerous, as to preclude all hope of turning these facts to any practical account. The best application of which they admit is to enforce the necessity of assuming nothing in regard to the functions of the human body in health or disease, and of submitting every question as it arises to the test of observation.

Zimmermann [*] evidently assumes that the pulse in disease is the reverse of that which obtains in health, when he makes the following prognosis. "If," says he, "the number of strokes be much greater in the morning than it ought to be in health, we may expect that the ensuing night will be a bad one. But if the pulse, instead of augmenting in quickness, becomes slower at night, we may conclude that the disease is on the decline." Here Zimmermann not only assumes that the pulse in health is less frequent in the morning than at night, but that the reverse naturally, and, as it were of necessity, obtains in disease. Dr Knox seems to have fallen into the same error in applying the results of his own observations. "It was," he observes, "from a sinking of the pulse towards evening that I ventured, independent of other circumstances, to prognosticate favourably in the case of a child labouring under typhus fever. It was this which induced me to hope that some of the functions had begun to resume their natural order, and that recovery was a probable event. Nor was I deceived. By proper attention the patient from that day rapidly amended."* There is no doubt that the sinking of the pulse, at whatever time of the day it occur, is a good sign. But the facts which I have recorded show, that the time of the day at which it occurs, cannot reasonably form an element in the prognosis.

---

[*] Experience in Physic, Vol. i. p. 250.

Diurnal Variations of the Pulse in Disease.

TABLE I.—MALES.

Disease.	Age.	MORNING.				EVENING.			
		Obs.	Max.	Min.	Mean.	Obs.	Max.	Min.	Mean.
Phthisis, .	28	5	93	86	89	5	90	76	82
Ibid. .	25	3	102	92	97	3	95	86	91
Ibid. .	36	7	109	94	100	6	108	84	94
Ibid. .	46	15	114	76	97	14	128	84	103
Bright kidney, .	28	5	90	70	77	5	90	60	72
Ibid. .	50	5	74	64	70	5	73	60	66
Ibid. .	40	5	98	83	87	5	96	84	88
Acute rheumatism,	64	2	78	68	73	2	78	62	70
Ibid. convalescent,	30	3	83	80	81	3	82	78	80
Ibid. .	16	5	85	67	77	5	92	72	81
Subacute rheumat.	14	5	80	71	76	5	88	68	77
Paralysis, .	31	4	107	70	93	4	76	65	71
Paralysis agitans,	22	4	72	63	67	4	78	67	73
Loss of power, .	64	5	76	64	70	4	69	58	64
Disease of brain,	39	2	110	80	95	2	98	83	91
Cephalalgia, .	19	5	86	76	80	5	96	67	79
Transposn. of heart,	67	15	60	44	49	14	60	41	48
Erysipelas. .	39	14	108	82	97	13	105	83	97

TABLE II.—FEMALES.

Disease.	Age.	MORNING.				EVENING.			
		Obs.	Max.	Min.	Mean.	Obs.	Max.	Min.	Mean.
Phthisis, .	35	5	134	102	115	4	122	96	107
Fever, .	26	3	76	57	69	3	84	60	69
Acute rheumatism,	16	3	76	64	72	3	84	70	75
Paralysis, .	22	5	120	104	112	5	116	100	106
Chorea, .	9	5	99	76	84	5	98	68	82
Idem, .	12	5	106	84	94	5	122	96	106
Chronic bronchitis,	63	5	87	77	82	5	92	78	84
Hæmatemesis, .	34	5	100	87	92	5	120	92	105
Colic. Muscul. rhm.	42	3	74	65	67	3	78	68	73
Periostitis, .	24	5	93	79	85	5	96	82	88
Erythema nodosum,	22	5	95	79	88	5	118	90	102

---

* Edinburgh Medical and Surgical Journal, Vol. xi. p. 06.

Disease.	Age.	Morning.				Evening.			
		Obs.	Max.	Min.	Mean.	Obs.	Max.	Min.	Mean.
Secondary syphilis,	22	5	98	64	81	5	96	70	86
Dyspepsia, .	21	4	87	72	79	4	102	84	94
Hysteria, .	26	3	61	57	59	3	58	56	57
Id. .	19	3	67	61	64	3	70	68	69
Id. .	31	3	93	87	91	3	92	88	90
Id. .	26	5	79	67	73	5	78	70	75
Hyster. paraplegia,	30	5	89	78	81	5	89	72	82

ART. X.—*Observations on the Use of Piperine in the Treatment of Intermittent Fevers.* By ROBERT HARTLE, M.D., Member of the Royal College of Surgeons, London, H.P. Deputy Inspector-General of Army Hospitals. (Read before the Medico-Chirurgical Society of Edinburgh on the 8th December 1840.)

INTERMITTENT fever is very common in this island and its town; yet this town of Port of Spain is the best laid out and the cleanest in the West Indies; but, as Dr Fergusson in his description of Trinidad states, the grand Savannah lies to the south-east of Port of Spain, and extends to its south-eastern borders, I am induced to believe that the marsh has in the dry season, or immediately on the rains commencing, some influence over the inhabitants, and more particularly over the residents of that part called Piccadilly, which is subjacent to its influence; for very few of those residing in this district are exempt from the malady. We have no disease in which there is so much susceptibility of a renewal as in intermittent, and in this island it is invariably an insidious and obstinate disease to subdue.

I have particularly remarked that sulphate of quinine ofttimes failed to eradicate the malady, although I had given it in ten grain doses, frequently repeated, beginning as soon as the perspiration commenced, without reference to the heat or feverish feel of the skin. It therefore occurred to me that the piperine would best answer the purpose of breaking the catenation of morbid symptoms which constitute the paroxysms of ague, and that, when that was accomplished, by combining it with the sulphate of quinine, it would succeed in removing the disease.

Accordingly, in all the cases of long standing, (many of them with an enlargement of both liver and spleen,) I began, as soon as the sweating stage was established, by giving three grains of piperine every hour, until eighteen grains had been taken; and on the following day, when the intermission was complete, I gave the same quantity every three hours. It has in every case succeeded in checking the paroxysm, and as soon as that was accomplished, I gave for some days the following pills;

℞ *Pilulæ Hydrarg.* gr. i.; *Piperinæ*, gr. ii.; *Sulph. Quinæ*, gr. ii.; *Syrup com. q. s. f. M. f. Pil. no.* 1. *omni mane, meridie, et vespere capienda.*

I shall here subjoin a few cases of long standing intermittent, in which the piperine was given as soon as general perspiration commenced, without regard to the heat of skin or quickness of pulse.

1. Miss S., aged 18.—This young lady had suffered periodically upwards of two years with intermittent. It appeared that she had taken sulphate of quinine in great quantities and very large doses, with the effect of only subduing the disease for a short time. She had been removed to North America for change of climate, and every other means had been used to cure the disease, without success. Her father in conversation mentioned this to me, and I advised him to give the piperine; and, accordingly, gave him the following prescription, with directions to begin as soon as perspiration became general, and that, should she escape the second paroxysm, the pills were to be continued every third hour.

℞ *Piperinæ* gr. v.; *M. G. Acaciæ, q. s. f. M. f. Pil. no.* 1. *2da quaque hora sumenda.*

It is now upwards of three years, and she has continued free of the malady.

2. Mrs C. aged 30, had had several severe paroxysms of tertian intermittent, but very irregular in its attacks. Infusion of bark as well as sulphate of quinine had been freely taken, but to no purpose. On the morning of the 17th April 1837, while taking a cold bath, she was seized with ague. I was sent for at 7 o'clock A. M. I found her shivering severely. I ordered warm drinks to be given frequently. At 12 o'clock the cold shivering continued, and the symptoms were alarming. The pulse was scarcely to be felt. The coldness was universal, clammy, and unpleasant to the touch. The lips were of a slate colour. The countenance was contracted, the eyes sunken; and the patient evinced marks of great agitation and alarm.

I ordered hot flannels to be applied over the chest and abdomen, the legs and feet to be rolled up in hot cloths, and stone vessels filled with hot water to be constantly kept under her arms, and along the inside of the thighs and legs. Warm brandy toddy was ordered to be frequently given in small quantities. At 4 o'clock P. M., finding that reaction could not be brought on, I gave five grains of piperine, which was with difficulty swallowed; and I washed it down with an ounce of brandy, in half a tumbler of champagne. I then ordered full doses of the aromatic spirit of ammonia every third hour, and the champagne to be continued.

At 6 P. M. reaction commenced; and as the heat increased I withdrew the hot applications, and suspended the stimulants. By 8 o'clock at night the heat was general, and in proportion to the cold stage. The following morning the sweating stage commenced; and, as soon as it was general, I began with five-grain piperine pills every second hour, and continued them for four days. She had no return of the disease; and for a few days I continued to give them four a day. She has never had a return since.

3. Mr W. aged 27. This young gentleman, a European, had, soon after his arrival in this island, about a year and a-half ago, a severe attack of remittent fever, which ended in tertian intermittent. He had taken a quantity of sulphate of quinine, which for a short time checked the paroxysms; but for three months, it had constantly returned every forty-eight hours, and so reduced his strength that he was unable to perform the duties of his occupation. In consequence, his employer directed him to call on me for advice. On examining him, I found his liver and spleen enlarged and hard, yet free from pain when pressed on. I ordered the following pills;—

℞ *Piperinæ*, gr. xxxvj.; *Pilulæ Hydrarg.*, gr. xij. *M. f. Massa in pilulas* xij. *distribuenda. Quarum capiat unam 2da quaque hora.*

And in case the bowels were not free, he was to take the following mixture;—

℞ *Sulph. Magnesiæ*, ℥iss.; *Sulph. Quiniæ*, ℈ij.; *Aq. puræ*, ℥xx.; *Acid. Sulph. Dil.* ʒij. *M. Hab. Cyathum pro re nata.*

It is now three months since he had an attack of the disease, and his health is perfectly restored.

I must here observe, that a number of patients object to taking the sulphate of quinine, in consequence of its affecting the head; but the piperine, although a powerful stimulant, carminative, and febrifuge, does not in the least degree affect the sensorium.

Port of Spain, Trinidad, 21st Sept. 1840.

Art. XI.—*Observations on the Therapeutic Action of Croton Oil in certain Nervous Disorders.* By P. S. K. Newbigging, M. D., F. R. C. S. E., one of the Medical Officers of the New Town Dispensary, &c. &c.

The cases in which croton oil has been chiefly administered have been those where an active purgative was considered necessary, the smallness of its dose allowing it to be given in circumstances where other effectual medicines could not be employed. Although mention is made by Dr Conwell of his having used the oil of the Croton tiglium in a case of obstinate constipation, in an individual affected with *tic doulourenx*, it was not supposed to possess any specific influence on this disease until lately, when Sir C. Bell brought it into notice by his successful treatment of such cases. I am aware that its specific effect on neuralgic complaints is by no means generally allowed by the profession, and that many instances may occur where this medicine can be of no avail, as in cases of organic disease. The very beneficial results, however, which have followed its administration in several cases of epilepsy, and of severe neuralgic pains connected with *tic douloureux*, which have come under my observation, both in this country and abroad, would seem to indicate, in my opinion, the possibility of there being some specific influence in the croton oil, apart from its merely purgative action. In the hope that, from an accumulation of facts, some data may be ultimately arrived at for determining this question, one of great importance, I have thought it proper to give a short statement of a few of the more marked of these cases. It is not my intention to enter fully into the details of each case. I shall content myself with mentioning generally its history and results.

My attention was first particularly called to the apparent specific action of croton oil, by a case under the charge of the late M. Lerminier of La Charité of Paris, in which a young female, owing to exposure to cold, had an attack of *tic douloureux*, for which she had been subjected to a great variety of treatment in the form of depletion, counter-irritation, &c. without relief, but which, as soon as the purging induced by the croton oil had commenced, yielded, and was entirely removed by a repetition of the dose. I afterwards had an opportunity of witnessing, in the practice of that eminent physician, the beneficial effects of this drug upon similar complaints. In one remarkable instance, the patient had for upwards of three years laboured under sciatica of the left leg, which was considerably diminished in size, and could not be used without great difficulty and increase of pain. The subject of this case ultimately left the hospital, cured as regarded the severe suffering, and although lame from deficient nutrition of the limb, still much less so than on his admission. In my practice at the New Town Dispensary of Edinburgh and otherwise, I have had frequent occasion to employ the croton oil, in cases indicating the administration of a purgative, and, induced by the cases which I had seen under the care of M. Lerminier, as well as by the favourable opinion entertained of this drug by authors, I employed it in the case of an individual aged 60, who had been subject to a severe form of epilepsy, recurring at intervals of six weeks during the last twelve years. Immediately after the attack for which I was requested to see him had passed off, I administered a drop and a half of croton oil, which produced vigorous purgation, and have continued the medicine in small doses occasionally since.

The result of this treatment, together with the regulation of diet, &c. has been, that since the 7th of October 1839 he has not had any return of his complaint. Encouraged by the happy effect of this treatment upon one of the most formidable and afflicting diseases, I resolved to employ it in similar cases. In December 1839, I was asked to visit a boy, 5 years of age, who had been epileptic for two years. The fits were at first at intervals of from three to four weeks, but at the time I was consulted they were as frequent as twice or thrice in the twenty-four hours, and fatuity was rapidly supervening. I administered some aloetic medicine, which produced considerable alvine evacuation, but without benefit; the fits were still violent, and were becoming more frequent, than when I first saw the patient. A drop of croton oil was given to the child, and on making my visit next day, I found it had produced several stools, and that since this action had been induced there had been no return of the fits. This favourable state continued, and the child gradually regained its vigour of mind and body, and ultimately got quite well.

I had occasion to be visiting in the same quarter six months after, and saw my patient. He had remained quite free from any attack, and was in every respect like a boy of his time of life.

In December of the same year, 1839, I was sent for to a female, aged 20, who had had three attacks of epilepsy previous to the one for which I was called to see her. The croton-oil was administered with the best results, and she has since had no recurrence of the malady.

It may be proper here to remark, that the form of epilepsy in which the croton-oil appears to me to be useful is that which probably arises from some irregularity in the cerebral circulation.

Those cases wherein I have been disappointed in producing decided benefit,—and such must always be numerous;—probably arise from organic disease in the brain or its meninges, where palliative treatment can alone be useful; but in these I have occasionally observed that the recurrence of the paroxysms are not so frequent, and are generally less severe after the use of this oil.

The next case which I have to state is that of a woman aged 50, who had been a sufferer from sciatica for three years, and been under medical treatment without deriving any perceptible benefit. I learned from her that local blood-letting, counter-irritation, setons, &c. had been employed without advantage; and when I and my friend Dr Philip Maclagan were called to see her, she was unable to leave her bed, and was so helpless as to require the constant assistance of another. We commenced the use of the oil externally as a counter-irritant, and administered it internally in a moderate dose morning and evening. This system she continued for some time, and in about a fortnight after its commencement she was enabled to leave her bed, and move about the house. The pain has now left her, and she complains only of want of power in the limbs.

In the following case, that of a gentleman who had, during a period of ten months, suffered great pain in the anterior and lower part of the right thigh, the pain was so acute, as to be supposed to have a periostitic origin, and leeching, fomentations, blisters, &c. were employed, and medicines given internally, without any alleviation of his suffering. Two drops of the oil were administered. Full action of the bowels with great nausea supervened, and the pain simultaneously left him; nor has it since returned

I had an opportunity of availing myself of the use of this remedy in a case of *laryngismus stridulus*, or crowing disease, which, from its supposed analogy to other nervous disorders, I conceived might be relieved by a similar treatment. The fits or spasms occurred in a child aged 9 months, and had existed for about a fortnight previous to my seeing him. I administered the antispasmodics usually employed in such cases, along with a properly regulated diet; but the frequency of the fits not appearing to be diminished, and having, on the contrary, increased to twenty-six in the twenty-four hours, I gave the child a quarter of a drop of the oil, which shortly after began to act. After this the paroxysms ceased, nor did the fits again return for several hours, and subsequently, they never exceeded four or five in the twenty-four hours, and in a few days the child was quite well.

The effect of the croton oil in this instance was very severe, and the patient was much reduced in consequence. I should, therefore, in any future case, combine the tonic treatment along with what would appear to be the specific action of this oil. It is not my intention at present to give a farther detail of cases, believing that those I have now stated go far to show that there is much reason to suppose that the croton oil possesses a specific influence, independent of its purgative action, in some forms of epilepsy and neuralgic cases. In this opinion I find that M. Andral coincides,* as he states that he has observed great benefit arising from the employment of this oil in neuralgic complaints, when purging ensued; whereas a similar action on the bowels induced by other medicines was not followed by a like result.

It has been ascertained by† M. Brandt that croton oil contains, amongst many other ingredients, a peculiar acid, to which he has given the name crotonic; but whether this be the source of what appears to be the specific action upon the nervous system, which I have endeavoured to illustrate, it would be difficult to affirm.

---

* Clinique Medicale, Vol. v.

† Journal de Pharmacie, Tome xi.

The subject, as I have already stated, appears to me well worthy of investigation.

ART. XII.—*Case of Amputation of the Neck of the Womb followed by Pregnancy; with Remarks on the Pathology and Radical Treatment of the Cauliflower Excrescence from the Os Uteri.* By JAMES Y. SIMPSON, M. D., Professor of Midwifery in the University of Edinburgh.

IN his learned work on the Diseases of Females, (Dublin, 1838,) Dr Churchill remarks, (p. 249,) "I am not aware that any attempts have been made in Great Britain to excise the *cervix uteri*."

The following instance of this operation may therefore not be uninteresting, either as regards its details, or the hitherto flattering success that has resulted from it.

In the beginning of May last I was requested by Dr Lewins of Leith, to visit with him Mrs Cameron, who, as he informed me, had a tumour attached to the *cervix uteri*.

The patient, aged 33, had been married for thirteen years. During that period she had borne five living children, and suffered from a miscarriage at the sixth month. In June 1838, she weaned her youngest child. For about a month previously to that date she had a red discharge from the vagina, which was constant in its occurrence, though not great in its quantity. It continued during the autumn. In October, she passed with labour pains of three or four hours duration, a body which the midwife in attendance supposed to be an abortion of the second month. During the period of pregnancy with this alleged abortion, the vaginal discharge was still present. It increased considerably after October, and was now often mixed with coagula of blood. It had always a very offensive smell and more or less of a red tint, but sometimes it appeared comparatively pale and watery. The discharge was as profuse though less discoloured during the night, and when at rest, as during the day and when taking free exercise. From the supposed period of abortion in October, up to the period that I saw her with Dr Lewins in May, three or four cloths were soaked regularly every twenty-four hours by it. Whenever she ventured to walk about without napkins she felt the discharge "running" (to use her own expression) from her. On two separate occasions the escape of pure blood became suddenly so

Edinr Med. & Surg Jour. Vol.LV. Plate 6.P.

Dr Simpson's Case of Excision of Cervix Uteri

FIG. 1.

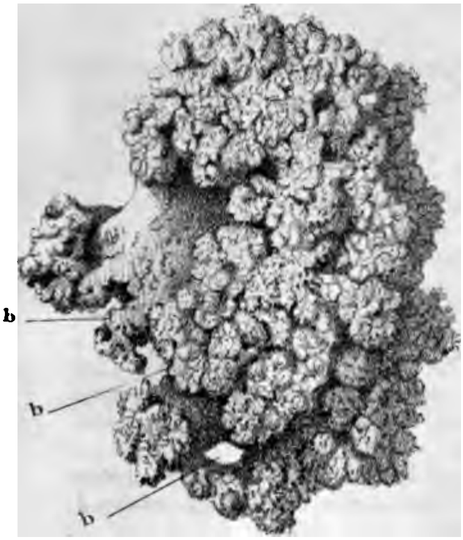


FIG. 3.

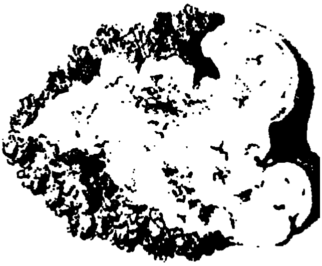


FIG. 2.

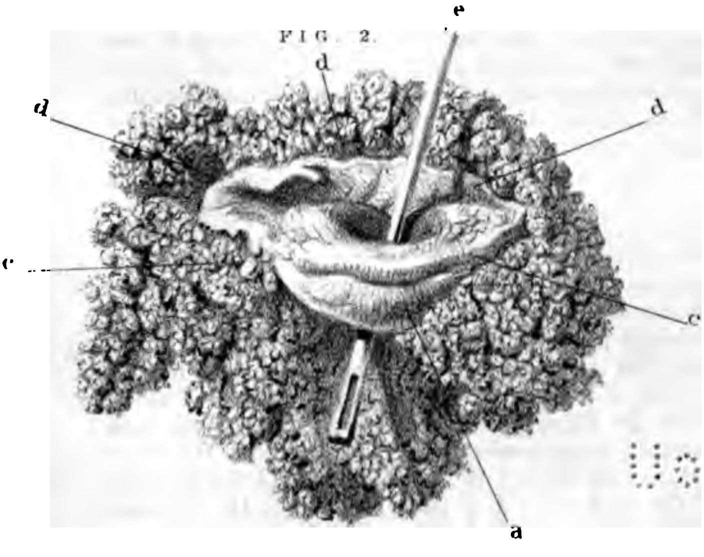
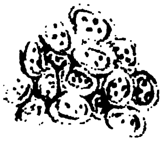


FIG. 4.



UofM

MtoU

great as to pass through all the cloths and create great alarm. Mrs C. was not aware of any causes which excited these attacks of hæmorrhage. One of them occurred during the night. She never observed any monthly increase in the discharge answering to the catamenial periods.

During the whole course of the disease Mrs C. had not suffered (if we except the temporary expulsive uterine action in October) any pain or uneasiness whatever in the region of the uterus; but by the time that I first saw her she had become greatly weakened and reduced by the abundant discharges. Her face was pale and anæmic, and she was occasionally obliged to keep her bed in consequence of debility and exhaustion.

Dr Lewins was first called in to see the patient a short time previously to my visiting her along with him. On examination *per vaginam* I found, as Dr Lewins had described to me, a tumour fixed to the posterior lip of the uterus. It was then about the size of a small pear, and was attached by a very broad basis. The surface of the tumour felt somewhat rugged and granulated. It was firm but not hard in its consistence. The patient did not complain of any pain upon touching or pressing its surface. Its superficial vessels bled freely under every attempt at examination. On introducing the *speculum vaginæ*, and embracing the diseased mass within the further extremity of the instrument, the surface of the tumour was seen to be irregular, and of a bright-red, strawberry colour.*

---

* It is almost unnecessary, we believe, to insist at the present day, upon the importance of the early and accurate local examination of the uterus in all cases of suspicious vaginal discharges. In some instances, examination by the finger may be sufficient, but in every doubtful case the speculum should likewise be resorted to if there is any affection of the vagina or cervix. We have found it often confirming, and not unfrequently also changing and rectifying the opinion which the mere tactile examination had led us to adopt. In this country great difficulties have been placed against the more general introduction of the speculum into practice in consequence of the disagreeable and revolting exposure of the person of the patient, which is usually considered necessary in its employment. We have latterly in our own practice endeavoured to avoid this very natural objection, by teaching ourselves to introduce and use the instrument when the patient was placed on her left side in the position usually assumed in making a tactile examination, and with the nates near the edge of the bed. We strongly recommend our professional brethren to follow this plan, as by it, and with attention to the management of the bed-clothes, we have found that the instrument can be perfectly employed with little, or indeed without any exposure of the body of the patient. The speculum is introduced easily without the assistance of sight, and the mouth of it only requires to be afterwards uncovered, in order to enable us to examine the *cervix uteri* and top of the vagina. We have made trials of many different forms of specula, and find, for almost all purposes, that of Ricord by far the most manageable. In exposing the *cervix uteri* for the purpose of drawing blood from it by scarifications, in cases of chronic congestion and metritis, we have occasionally employed a tubular speculum with advantage, but even in this case the double-bladed instrument is equally useful, and in some instances preferable. In a case of ulcer of the *os uteri* which we are at present attending with Dr John Gairdner, and where the passages are much relaxed and the uterus very low in the vagina, we have, on Dr Gairdner's suggestion, employed with much

It appeared possible to grasp the basis of the tumour with a ligature; but both Dr Lewins and I were of opinion that the free amputation of the *cervix uteri*, with the diseased structure attached to it, offered by far the most probable means of success. We communicated this opinion to the patient's husband, and at the same time stated, that, even under this method of treatment, the disease would probably recur. After a delay of about three weeks, Mrs C. announced that she was ready to submit to the operation that we had proposed. In making a re-examination after that short interval, I was perfectly convinced that the excrescence had grown considerably, and was extended in its base so as to involve more of the angles of the *os uteri*, as well as of its posterior lip.

On the 25th May, I proceeded to excise the *cervix uteri*, and was assisted in the operation by Dr Lewins and Mr Ziegler.

The patient was laid upon her face, her body placed across the bed, and her lower extremities allowed to hang over the front of it. The thighs were held separate from one another. My object was to pull down the diseased neck of the uterus till it protruded externally beyond the mouth of the vagina, and then freely excise it. For this purpose I introduced the two first fingers of my left hand into the vaginal canal up as far as the tumour, and used them as a guide by which I fixed the teeth of a long vulcellum into the sides of the excrescence. Its tissue, however, was so soft as to tear under slight traction, and thus afford me little purchase for pulling the mass downwards. The instrument was refixed nearer the root of the excrescence, and a second vulcellum was superadded to render the purchase the more secure. With these I was enabled to pull down the tumour gradually and cautiously until it was entirely protruded beyond the external parts. Dr Lewins and Mr Ziegler having satisfied themselves that the *cervix uteri* and whole bulk of the tumour was extruded, I cut off the protruded mass, dividing it from behind forwards, and removing the whole vaginal portion of the *cervix uteri*. The uterus immediately slipped up into its natural position. Very little hemorrhage followed. I stuffed, however, the vagina pretty firmly, under the fear that dangerous bleeding might supervene.

The patient bore the operation well, and complained wonderfully little during it. In the evening, Dr Lewins had to remove the vaginal plug, in order to allow her to evacuate the bladder. It was not considered necessary to replace it. No marked morbid symptoms whatever, either local or constitutional, fol-

---

advantage a short tubular speculum of only an inch and a-half in length, and with a deficiency or opening along the course of one side of it, of sufficient size to enable us to pass our finger for the purpose of placing the diseased part in the proper centre of the instrument. We have thus been enabled to touch easily the ulcerated surface with different applications; while with the usual instruments it was found a very difficult task to fix in this instance the very mobile *cervix uteri*.

lowed. The great vaginal discharge immediately ceased. On being interrogated in relation to this point on the second day, the patient emphatically described herself to Dr Lewins as "*quite dry*," and that for the first time for many months. The incised surface, when examined through the speculum a few days after the operation, presented a healthy granulating appearance. It was not considered advisable to allow her to sit up till the tenth day after the operation was performed, and in a few days more she began to walk about the house and perform her usual domestic duties.

She has not been one hour sick since the period of the operation, and has now regained her usual strength and spirits.

No morbid discharge from the vagina of any kind has hitherto appeared. She has never since menstruated; and about five weeks ago she fancied that she felt the symptoms of quickening. On examining the abdomen to-day (14th November) with the stethoscope, I heard distinctly both the placental souffle and the sounds of the fœtal heart. The *os uteri* is closed, and on examination by the finger, gives the sensation of a firm puckered cicatrix.

The excrescence after its removal was found to measure two inches and three-quarters at its broadest part, and two inches and a quarter at its greatest depth. The thickness of it where it implicated the posterior lip of the *os uteri* was one and three-eighths of an inch, but on either side it stretched forward, and involved the angle between the anterior and posterior lips; thus rendering this admeasurement greater on its lateral parts. The anterior lip of the *os uteri*, (see Plate, VI. Fig. 2. *a.*) which was fully removed as high as the reflection of the vagina, seemed sound except at the above angles. The posterior surface (Fig. 1.) of the posterior lip was densely and completely covered by the excrescence, up to the reflection upon the vagina.* In excising the diseased part, I removed it so high as to bring away all around, a small portion of the reflection itself of the mucous membrane of the vagina. The surface of this portion of membrane, as thus removed in attachment to the upper edge of the excrescence, appeared quite healthy on careful examination of the excised mass. (See Fig. 2, *d*, *d*, *d*.) The surface of the tumour presented a well-marked small granulated appearance with deeper fissures crossing it, and giving it an irregular and lobulated appearance. The sides of it were considerably and deeply lacerated (*b*, *b*, *b*,) in various places by the teeth of the vulcellum. On rubbing down any small part of the recent tumour between the finger and thumb, a kind of vascular or cellular frame-work was all that was left behind. The mass, before dividing it, was steeped in a strong alcoholic solution of corrosive sublimate, in order to insure its preservation. On making a section of the tumour after it had been thus sufficiently indurated, it presented to the touch and sight an appearance greatly resembling that of the brain when hardened by the same menstruum. A number of minute cells are scattered over the surface of the section. On slightly rubbing any part of the section (see Fig. 3,) but particularly the more external part of it with the handle of the scalpel, its apparently homogeneous structure at once breaks up and revolves itself into an immense number of very small connected grape-like granules. These same granules impart to the external surface of the excrescence its peculiar minutely mammillated structure; while their arrangement into nodules, in consequence of the divided and lobulated arrangement of the superficies of the tumour, gives to the whole a striking resemblance to the head of the cauliflower. The accompanying drawings of the tumour by my friend Dr Paterson give excellent representations of its external form.

---

* *C*, *c*, (Fig. 2.) the surface of the incision; *e*, a probe passed through the *os uteri* and canal of the cervix.

On submitting some very thin slices from the surface of the section of the tumour, (Fig. 3.) to a powerful microscope in the possession of Dr Reid, it was seen to be composed of a number of cells arranged in some places in groups, in others in irregular lines. These cells contained each a large nucleus, and this nucleus inclosed several small nucleoli. The structure in question of cells or cystoblasts, incasing nuclei and nucleoli, has been shown to be so common as an elementary form of natural structure, by Schleiden and Schwann, and as an elementary form of various morbid tissue by Valentin, Gluge, and Muller, that no conclusion, in the present state of our knowledge, can be positively drawn from this microscopic structure alone. But it may be interesting to add, that none of the caudate or spindle-shaped bodies described by Müller as often existing in morbid encephaloid structures were seen in any section that was examined. The microscopic appearance of the compound cell-globules constituting the granules, and composing the mass of the excrescence, are well represented in the drawing, (Fig. 4,) kindly made for me by Mr Goodsir.

Pathological Nature of Cauliflower Excrescence.—The history, symptoms, physical characters, and minute structure, of the preceding tumour appear to refer it indubitably to that species of growth which was first accurately distinguished and described by Dr Clarke, under the quaint but expressive name of the " Cauliflower Excrescence from the *os uteri*."*

The pathological nature of this variety of morbid growth has given rise to considerable difference of opinion among physicians. Drs Gooch, Hooper, Davis, and Lee, regard it as truly cancerous in its character. Others, as Drs Clarke, Burns, and Waller, consider it as a morbid tissue, not necessarily of a malignant or carcinomatous nature. A number of circumstances appear to me to show, that in reference to, at least, the first stage of cauliflower excrescence, the opinion of these latter authors is probably correct. The occurrence of the disease in some cases as early as the 20th year of life;*—its occasional shrinking and almost total disappearance upon the application of a ligature, or after death;†—the frequent slowness of its general progress during life; the apparent absence of diseased deposits in the neighbouring tissues and parts upon the dead body;‡—and, above all, the alleged restriction and even complete removal of the tumour, in one or two instances, by the use of astringent applications and other simple means,§ form so many circumstances strongly pointing to the opinion that in the earlier part of its progress the tumour cannot be regarded as of a carcinomatous character.

---

* See Transactions of a Society for the Improvement of Medical and Chirurgical Knowledge, Vol. iii. p. 321. (1809.)

Has it any analogy in its pathological nature and origin—as it certainly has in its physical characters—with the soft warts and condylomata that sometimes form on the mucous membrane of the vulva and entrance of the vagina? These warts and condylomata have the same tendency to regeneration after their imperfect removal, and present to us a striking exception to the general pathological law of the local reproduction of a morbid growth being a sign of its malignancy.

But, whatever view we may take of the primary nature of the cauliflower excrescence of the *cervix uteri*, we have sufficient evidence for believing either that this disease has been often confounded with carcinomatous or medullary fungus from the *cervix uteri*, from the want of adequate diagnostic marks to distinguish them; or that, though non-malignant in its commencement, the cauliflower excrescence may, like some other local benign growths, become the seat of carcinomatous deposit and malignant action, during its progress. Thus it has been found by Gooch‖ and Madame Boivin¶ to return again in a malignant form, after its imperfect removal by the ligature or knife. In an instance mentioned by Dr Davis,** its removal was followed, after the lapse of a considerable period, by its reproduction, and ultimately by carcinomatous ulceration; and in two cases that occurred to Professors

---

* Sir C. M. Clarke on the Diseases of Females, Vol. ii. p. 62.

† Ibid. p. 70 and 75. ‡ Ibid. p. 66 and 70.

§ Ibid. p. 105 and 108. A lady, aged 32, had a continued profuse watery discharge mixed occasionally with blood. She was greatly weakened, pale and emaciated. A cauliflower mass projected from the surface of the *os uteri*. Several remedies, with cupping and local astringents, were ordered and assiduously persevered in. After two years, "*no difference* could be felt between the *os uteri* of the patient and that of a woman in perfect health." (Clarke, p. 107.)

‖ On the most important Diseases peculiar to Women, p. 288.

¶ Heming's translation of Boivin and Duges' work, p. 300.

** Principles of Obstetric Medicine, Vol. ii. p. 744.

D'Outrepont* and Siebold,† in which large tumours having a cauliflower form were found affixed to the *cervix uteri* during parturition, the neighbouring uterine tissues, as well as the contiguous structures of the bladder and uterus, were found in a carcinomatous state upon the *post mortem* dissection. In another case, in which Michaelis‡ excised what he terms a *fungus medullaris* with a cauliflower appearance, from the anterior lip of the uterus during labour, the posterior lip of the organ afterwards degenerated, and cancer of the stomach ultimately supervened.

If these latter cases were not merely more advanced stages of the cauliflower excrescence, but, as appears to us not improbable, diseases originally and pathologically different from it, though resembling true cauliflower excrescence in its peculiar form and external physical characters, are there any means which might enable us to form a diagnosis between the two affections? The whole subject is one certainly demanding more careful observation and deeper investigation. The nature and characters, both physical and chemical, of the vaginal discharges in these and other maladies of the sexual parts, require to be more accurately examined and discriminated. May the degree of mobility of the *cervix uteri* serve in any case as a source of diagnosis? "The tendency of cancer, (as observed by Muller,§) is to interfere with the natural structure of surrounding parts, while those formations which are of a benignant nature leave the neighbouring healthy tissues unaltered." In carcinoma of the *cervix uteri*, we thus generally find, at even a pretty early stage of the disease, that the organ has become more *fixed* and immoveable than natural, in consequence of the morbid deposit affecting both the structure of the neck of the organ and the contiguous surrounding tissues. Does the reverse of this hold good with regard to cauliflower excrescence of the *cervix uteri*?

Radical Treatment of Cauliflower Excrescence.—Different measures have been proposed for the radical removal of cauliflower excrescences from the *cervix uteri*. The caustic, ligature, and knife have each been employed. With regard to the two former it seems superfluous to hope that the good results following upon their use can be more than temporary. The basis of the diseased structure will in all probability be left. Occasionally both the caustic and the ligature appeared to have produced injury rather than good, by the irritation and increased action that they have excited in the diseased parts.

If any radical operation and cure for cauliflower excrescence be attempted, the excision of the tumour with the whole of the vaginal portion of the *cervix uteri*, to which it is attached as a basis, appears to us to be the only measure which can at all be hoped to insure ultimate success. The disease has no doubt recurred in repeated instances even after this operation. In some of these cases it probably had advanced too far onwards to a carcinomatous character. In others the failure might be attributable (as confessed by Boivin and Duges, in regard to the cases which they themselves report,) to " the tumour being alone removed," and not the *cervix uteri* also, which forms its seat, and " is always more or less affected."* In a few authenticated cases on record, in which complete amputation of the *cervix uteri* with the attached tumour was performed, the patient was known to have remained free from any symptoms of the disease for several years afterwards. A search through the medical literature of the last twenty years would, in all probability, enable us to adduce several such instances; but it may be sufficient for our present purpose to adduce three cases, of which we have the notes lying before us, and that appear to us, as far as we can judge from the details and expressions of the reporters, to have been probable instances of the same species of tumour that Dr Clarke originally described.

---

* Abhandlungen Geburtshulflichen Inhalts, Th. i. p. 276.

† Dissertatio sistens casum singularem carcinomatis uteri cum graviditate conjuncti.

‡ Neue Zeitschrift-fuer Geburtskunde, Bd. iv. S. 176.

§ On the Nature and Structural Characteristics of Cancer, West's translation, p. 66.

CASE 1.—In an instance of what is termed fungous cancer (*cancer fongeux*) by Colombat,† that surgeon amputated the *cervix uteri* on the 2d June 1830. The wound completely cicatrized, and the patient's health was re-established. She died in April 1832 of epidemic cholera.

The fungous cancer, Colombat observes in another part of his work, (p. 711,) is one of the forms of cancer which is the least liable to return after excision of the parts.

CASE 2.—Boivin and Duges mention a case of cauliflower excrescence of more than two inches in diameter, which was attached to the anterior lip of the *cervix uteri*. It was removed, along with more than six lines of the *cervix uteri*, in November 1828. The patient was alive in October 1832, and is then reported by the above authors as only labouring under some symptom of menorrhagia and dysmenorrhœa at the menstrual periods.‡

CASE 3.—An instance is reported by Duparcque, under the head of " Exuberance de l'Uterus," in which Hervez de Chegoin excised the two lips of the uterus, affected with what the operator terms " a granular strawberry inflammation," and which he alleges has often been confounded with cancer. The discharge and other symptoms of the disease had been present two years previously to the operation. At the date of the report (four years after the excision of the diseased part) the patient remained perfectly well.

---

* Heming's Translation, p. 301.

† Colombat de l'Isere; Traité des Maladies des Femmes, Tom. ii. p. 701.

‡ See Heming's Translation of Boivin and Duges' Treatise on Diseases of the Uterus, p. 300–301, and drawings of the excrescence in the Atlas, Pl. xxiv. fig. 3 and 4.

In the case of Mrs C. which I have above reported, I undertook the amputation of the diseased part with, as has been already said, strong doubts as to its ultimate success. The patient's peace of mind was broken, and her constitution was so rapidly breaking down under the constant, profuse, and weakening discharges which afflicted her, that she would in all probability have soon sunk under them. Immediately after the operation was performed these discharges completely ceased, and have never since returned. Her health and strength have been in the meantime restored to her; and she is at the present moment, as I have already shown, advanced beyond the middle period of pregnancy. The morbid characters of the diseased structure that I removed are such certainly as to render its future regeneration not at all improbable; but as yet there are no local appearances of its return; and,—taking the very worst view of the case,—there seems to be no reasonable doubt but that the operation has restored the bodily comfort, and prolonged the life of the patient, if it has not entirely freed her from the risk of a future return of the disease.

ART. XIII.—*On Sanguineous Tumours on the Scalp in New-born Children.* By FRANCIS BLACK, M. D. &c.

MY attention was first drawn to this subject, from having under my care a patient with one of these tumours. On examining the case I was embarrassed, nor could I find anything satisfactory regarding it in any English work on midwifery. Dr Burns merely says, "children may, especially after tedious labour be born with a circumscribed swelling on the head. This seems to contain a fluid, and has so well defined hard edges, that one who for the first time saw a case of it, would suppose that the bone was deficient. It requires no particular treatment."*

I have attempted in this paper to collect all the information which the researches of continental practitioners have afforded, and trust that it may be a means of inducing British medical men to elucidate further this interesting subject.

Sanguineous tumours may be classed under three different heads; first, those in which the blood is effused under the integuments exterior to the pericranium; the second, that which is found between the pericranium and the bone; and the third species situated deeper than either of the preceding two, between the *dura mater* and bone. These tumours are not confined to the head, but are sometimes met with on the face, breech and shoulders, or on any other region which has suffered pressure in the pelvis during labour. But the head is their principal seat and where, from the peculiar nature of the coverings, they present distinctive characters, and often formidable symptoms. These tumours seldom discolour the incumbent skin, which presents a pale shining appearance; and generally the part is so little sensible that considerable pressure causes hardly any uneasiness. The child is sometimes born with them, or they appear a few days after birth, but a day or two, or even three may elapse before they are visible. In one case they may be stationary, retaining the same size which they had when first observed; or they may go on gradually increasing, owing to the extremities of the ruptured vessels being still open, and pouring out blood. We generally find them on the right parietal bone, seldom more than one, but in some instances two or three, either isolated or communicating with each other. The fluid which these tumours contain possesses more or less of the characters of blood; sometimes coagulated, in other cases fluid, and of a florid or venous colour, and occasionally so thin that it seems to be simply the serum of the blood mixed with the colouring matter. In cases of long standing there has been found an admixture of purulent matter.*

---

* Burns, 6th edit. p. 607.

The first class may be divided into two varieties, first, where the effusion is supra-aponeurotic; second, where it is sub-aponeurotic.

Supra-aponeurotic.—The first variety, where the blood is effused between the aponeurosis of the *occipito-frontalis* and the integuments, is the most common and least dangerous. The tumour is flat and irregular, losing itself in the surrounding tissues, pitting upon pressure, with fluctuation very obscure or entirely wanting; the skin more or less discoloured; and the hard projecting edge met with in the other varieties never presenting itself. This tumour has been called by the Germans *caput succedaneum*, and is not a sanguineous but a sero-sanguineous effusion. It differs from the other varieties in this respect, that it is not confined to one part of the cranium; that it very soon disappears; and is generally the consequence of pressure during delivery.

No other treatment is required than the application of some discutient lotion. Dr Geddings, however, says of this variety, "that they may be readily cured by freely evacuating their contents,"† a point not to be doubted; but why should we even resort to severe measures, when gentler ones are always successful?

Sub-aponeurotic.—The second variety, where the extravasation is between the aponeurosis of the *occipito-frontalis* and the pericranium, differs in many points from the last mentioned. It has been described by Baudelocque‡ Velpeau, and Geddings; but Valleix § states that he has only met with it in two cases, where the tumours were the effect of external violence; and concludes, from the statements of Naegele, Zeller, and Hoere, who deny altogether its existence, that it is exceedingly rare. The evidence of the three last authors is only negative; whereas that of the first mentioned and my own is positive.

---

* From the observations and experiments of Mr Gulliver, there is strong reason to believe that the material which resembles purulent matter in these tumours is rather dissolved fibrin. See Medico-Chirurgical Trans. Vol. xxii. p. 151.—EDITOR.

† American Journal of Medical Science, No. 46. p. 374.

‡ Baudelocque, Art des Accouchemens, 1re partie, Chap. 2.

§ Clinique des Maladies des Enfans, p. 497.

In a case which I treated, it occurred on the left parietal bone of a child which had been easily born. The tumour was round and fluctuating, of a doughy feel, but not presenting the projecting osseous circle, and without discoloration of the skin. Where the tumour ceased, there could be felt a hard surrounding border, but not conveying the sensation of the osseous circle peculiar to the *cephalæmatoma*, but a hardening of the soft parts. This tumour continued to increase for about a week, but never could I discover the osseous circle, and when completely cured, it left behind it no callus. From these appearances, I think I am warranted in concluding that this was a case of sub-aponeurotic tumour. On an infant's head which I dissected, I found a sub-aponeurotic tumour, where the effusion was more serous than sanguineous. Another case confirmed by dissection is also described by Vernois.

The sub-aponeurotic tumour, then, is roundish and fluctuating, not pitting to the touch, distinctly circumscribed, not by an osseous circle, but by an induration of the surrounding tissues. From the comparatively loose attachment of the aponeurosis to the pericranium, this tumour may cross a suture; and is larger and more diffused than the cephalæmatoma.

The treatment here ought to be a little more active than in the first variety: but the blood is generally absorbed of itself. If, however, after eight or ten days we see no diminution, lotions of muriate of ammonia, made more or less stimulating by alcohol, should be applied. This may be combined with pressure. Before using these we may puncture with a cataract needle, so as to draw off the contents.

What we dread in an obstinate case is that suppuration may be induced, or that from the continued irritation of the contents, the pericranium be separated from the bone.* It is remarkable that these tumours cause so little uneasiness. In the case I have before alluded to, the tumour, at first the size of a walnut, went on gradually increasing, until it extended from near the mastoid process to the posterior fontanelle. Notwithstanding this the child was otherwise in perfect health. It was punctured, more, I confess, to see the contents than for any useful end; no lotion was used, and it entirely disappeared in the course of a fortnight. In the case described by Vernois, an incision was made, and at the end of three days suppuration took place, but before a month the wound was cicatrized, and the child did not seem to have suffered. It afterwards died of pneumonia. Local bleeding can be of no use; nay, it may even hasten what we ought to prevent, viz. suppuration, or sloughing of the parts. Blisters are dangerous discutients, owing to the liability of mortification occurring,

Second Class. Cephalæmatoma.—Almost each author who has turned his attention to this tumour has employed a new name. Thus it has been styled *Ecchymoma capitis* by Feller and Carus;

---

* See fatal cases of Dr Stuart's, American Journ. of Med. No. 46. p. 375.

Plenck added the additional epithet of *cariosum* ;[*] Gölis calls it *Thrombus neonatorum ;* and Palletta *Abscessus sanguineus capitis*.[†] I shall employ the term Cephalæmatoma, introduced by Zeller,[‡] and now in general use.

The rareness of this disease will be seen from the fact that M. P. Dubois mentions, that at the Hôspice de la Maternité, where from 2500 to 3000 children are born every year, he only met with six cases. M. Baron gives as the proportion in the Hospital des Enfans Trouvés, two once in 400 cases : M. Valleix once in 387 ; and Hoere once in 100 cases.[§]

In this species the blood is extravasated between the bone and the pericranium. Formerly authors were not aware of the seat of it, hence the contradictory statements which they gave ; some stating that it was always fatal, others that it was of little consequence, and soon disappeared; Dr Geddings says, " It must be difficult during the life of the patient to distinguish this variety from the preceding, (viz. those under the integuments), and fortunately, this is not of much consequence as far as the first is concerned ; but it may be important to discriminate this form of sanguineous tumour, and those which form in the diploé, because it would be unsafe to puncture some of the latter."[||] The correctness of this statement, however, is questionable. Dr Geddings agrees with Michaelis, Palletta, and others, who say that the disease originates in the diploe : but the opinions of Velpeau, Naegele, and Valleix, seem more probable. Michaelis and his followers found their opinion on the appearances observed on dissection, viz. necrosis or caries of the bone, the external table being destroyed ; but this is no proof that the disease commences in the diploe, as these appearances may be an effect and not a cause. If, as they say, blood is poured out from a varicose state of the veins in the diploe, why does the tumour not go on continually increasing ? Why is it that the tumour when punctured, at first, within a day or two of its appearance, gives exit to arterial blood, and ceases as soon as its contents are emptied ? Knowing how difficult it is to arrest hemorrhage from a blood-vessel in the compact tissue of bone, ought we not to expect, that if it arises from an ulcerated vessel, it should flow and again fill the cavity ? Besides, in all the accurate dissections which are recorded, we find the bone quite healthy, only covered by a few rough osseous points. No doubt, if the blood be allowed to remain long enough, necrosis of the bone may take place ; but that is a rare occurrence.

This variety is peculiarly distinguished by having a particular hard edge or ring round its base ; it is much more prominent and more circumscribed than those mentioned before ; and the skin is not discoloured.

[*] Doct. de cogn. et curat. morborum infant.

[†] Observationes Pathologicæ, Cap. x. Milan, 1820.

[‡] De Cephalæmatomate, &c. Heidelberg, 1822.

[§] Dict. de Med. en 25 vol. Art. Cephalæmatome.

[||] American Journ. Med. Vol. xlvi.

The following two cases, accurately described by M. Valleix, gave a clear account of the morbid anatomy of this disease.*

CASE I.—A male child, of the usual size, and healthy, was left on the day it was born at the *crèche* of the *Hospital des Enfans Trouvés.* On the 3d of August, it was taken to the infirmary for some pustules, which appeared on different parts of the body, and for a tumour situated on the right side of the head. On inquiry it was found that the tumour had acquired its present size when the child was taken in. It was situated on the posterior and superior part of the right parietal bone, a little above and below the parietal eminence. The tumour, about the size of a goose's egg, was rounded and soft, presented distinct fluctuation, but no discoloration of skin. On drawing the finger round the base you felt a distinct projecting circular border, giving the sensation as if the parietal was perforated. A continued and strong pressure did not seem to affect the brain, or give the child uneasiness; no pulsation to be felt. On the 3d diarrhœa came on, and on the 13th the infant died. The tumour was apparently in the same state as when it entered.

Dissection.—The skin which covered the tumour having been cut crucially and with care, I exposed the aponeurosis. This was unaffected, and divided in the same manner to show the pericranium, which was detached from the bone and raised about ten lines above it, and appeared from the blood which lay below it to be of a brownish colour. A puncture made into this gave exit to about two and a-half ounces of a sanguineous liquid, inodorous, reddish, and mixed with whitish streaks. Having exposed the parietal, which formed the base of the tumour, I found it covered with a membranous layer, of the same colour as the fluid, which was raised with ease. In a single point of its extent, of the size of a shilling, above the parietal eminence, this membrane was redder, more adherent, and formed small shreds. In detaching this gently, you saw slender filaments uniting it to the parietal bone, which were torn little by little in drawing them. At the same spot the bone was rough, red, and projecting, presenting an irregular plate. The internal surface of the pericranium was soft and smooth to the touch. It appeared a little thickened and slightly red, adhering strongly to a narrow circular edge, which surrounded the base of the tumour. I tore these adhesions; the bony ridge remained fixed to the bone, and the pericranium at this point presented no trace of alteration. I then distinctly said that what is called an osseous circle was a genuine ring, a morbid production developed on the parietal bone; it was circular, raised nearly two lines, red, rough and triangular, adhering by its base to the bone. One of its faces turned towards the tumour was covered by the membrane I have described; the other, looking outwards was oblique, and furnished points of insertion for the pericranium, which was more adherent at this point than at other parts of the head. At the angle which these two faces formed, were seen two layers of membrane separating from each other, the one soft and without consistence, going to cover the bone, the other firm and resisting, to form the superior part of the cyst. The external table was in no way destroyed; the parietal bone was hard and perfectly ossified. No depression was to be remarked at the portion which corresponded to the tumour; and the summit of the osseous circle was raised as much above the parts not comprised in the tumour as those which formed its base. The traces of inflammation in the intestines indicated the cause of death.

---

* Gazette Medicale de Paris, Sept. 1834; to be found also in 26th vol. of the Encyclographie des Sciences Medicales.

Case II.—C. Français, born 30th July 1834, was brought the day afterwards to the *crèche*, where it remained until the 6th of August, without the tumour on its head being observed. I saw the child the same day it was sent to the infirmary, when I recognized a tumour on the right side of the head, towards the superior and posterior part of the parietal bone, of the size of half the fist, and raised more than an inch. It presented the same appearances as the last mentioned case, only the osseous circle was not so complete. On the 10th it was seized with diarrhœa, of which it died on the 15th. The tumour examined every day had remained quite stationary.

Dissection disclosed the following circumstances. The blood, weighing about three ounces and a half, was found coagulated. There was also the cellular bed adhering to the bone by small filaments, which were easily torn. Where those most adhered, the bone was covered with small osseous granulations, red and projecting, which by their aggregation formed several small plates. The pericranium was very adherent to the osseous circle. Neither in this case nor in the preceding did it present any trace of ossification.

The abnormal appearances present in this affection deserve notice.

Osseous Circle.—This circle consists of matter thrown out in order to form a boundary to the extravasation; a means which nature adopts in order to give strength, and prevent the pericranium from being further separated from the bone. It is evident from its shape that it is formed partly by the bone and partly by the pericranium. Michaelis states that this bony ring is a pathognomonic sign of this kind of tumour; but in this statement he has been opposed by Zeller, Naegele, and others, who say that they have often found this circle absent, and that the swelling raised in consequence of contusions of the head, which are soft and fluctuating in their centre, are also surrounded with a hard border. But the induration surrounding a contusion can scarcely, upon careful examination, be mistaken for it; besides, in contusion, where we have always pain upon pressure, the hardening of the surrounding tissues is comparatively quick, whereas in *cephalœmatoma* this sign does not show itself often until two or three days after the appearance of the tumour. It is a curious fact, that, as long as the tumour goes on increasing, no circle can be felt, but no sooner has it formed than the enlargement ceases.* This proves what has been before stated, that this osseous border is intended to guard against further separation of the pericranium. Michaelis is mistaken, when he says that it is caused by the external table being destroyed. Such a deficiency could not give to the margin the appearance of a triangular elevated edge. Neither can it be caused, as Zeller states, by the superincumbent blood preventing the bone contained within the tumour from being developed in the same ratio as the bone exterior to it. But to refute this, we have only to look at the circle which is contained within the cyst; and must it not therefore sustain the same pressure as any other part of the base; and besides M. Valleix states, that when he pared it off, the exterior bone was on the same plane as that contained within the cyst.

---

* Valleix, op. cit. p. 504. Fortin, Presse Medicale, 1837, No. ix. Dr Wigand, as quoted by Zeller, op. cit.

Membrane covering the Bone.—Palletta states that he has always found a false white gelatinous membrane covering the bone. On the other hand, Naegele, Zeller, and Hoere, make no mention of it. Valleix, whose account is confirmed by M. Velpeau, affirms that he has seen it in four different cases. Of its existence there can be little doubt; but whether it is formed by the cellular tissue lying under the pericranium, which is found adhering to the bone after maceration, or is an exudation of coagulable lymph secreted by the bone, is not ascertained. If I may be allowed to state my opinion, I would say it was a natural membrane, which may have acquired an unusual appearance from the circumstances in which it is situated.

Causes.—Pressure on the child's head during its passage through the pelvis has been generally assigned as the cause of all these tumours. But though this is the fact in the case of the subcutaneous or *caput succedaneum,* it cannot, from many circumstances, I think, ever be the cause of the true cephalæmatome. The objections against pressure on the head during its passage through the pelvis are the following:

1. M. Valepelière gives a case where a fœtus was still-born, and was known to be dead before birth, in which he found all the cavities full of blood, and a sanguineous tumour on the scalp. He referred it to apoplexy of the uterus.*

M. Billard received a fœtus four or five months old. The membranes were in no way torn; and the waters of the amnion, on account of their transparency, permitted him to see the fœtus, whose head was dependent and its feet raised. On the summit of the head was remarked a large sanguineous effusion. This was evidently not the effect of compression.

Schmidt has seen the fœtus *in utero* affected with cephalæmatoma.† M. Fortin discovered the existence of a cephalæmatoma upon a child before its birth.‡

The case I have reported from Brouisseaux shows that the origin of the tumour must have been before delivery.§

---

* Encyclographie des Sciences Medicales, Trans. Med. Vol. xvi.

† Traité des Maladies des Enfans, 3d edit. p. 58.

‡ Presse Medicale, No. 9, 1837. § See page 124.

2. These tumours have been seen where the breech or feet presented.

3. Almost always observed after easy deliveries.

4. If it arose from pressure in the passages, we should expect pain, a symptom never absent in contusion.

Various theories have been brought forward, such as an abnormal condition of the blood-vessels, ulceration, *nævus maternus*, &c.; but as these are not supported by facts, we need not dwell on them. The most plausible yet proposed is that suggested by Dubois, and confirmed by Valleix. In order to follow fully their reasoning, it will be necessary for a moment to advert to the anatomy of the parietal bone.

The ossification of the parietal bone commences from one point in its centre, from which bony radii are seen shooting in all directions. At birth, all the portion beneath the parietal prominence is found completely ossified, and showing no radiated appearance; but above the prominence we find that ossification is not complete, and that the radiated appearance is most distinct. On examining the external surface of the dried bone, there are seen a number of fissures between the radii; a few of which immediately above the prominence are also open on the internal surface. In the fresh state, this bone is exceedingly vascular, and on removing the pericranium, which is done with great facility, in proportion as the bone is vascular, we see an immense number of minute vessels running in these fissures. On applying pressure round the circumference of the bone, the radii are removed one from the other, and the vessels are torn, the consequence of which is ecchymosis. Valleix after repeated dissections states that there is frequently ecchymosis on the superior part of the head in new-born children. The following are the data which he communicated to the Anatomical Society of Paris. In twenty-eight children, aged from five to forty hours, there was in seventeen an ecchymosis on the parietal bone; eleven of these were on the right parietal: in two the ecchymosis covered to the same extent both parietals; in two others it was larger upon the left, and in the last two it was so circumscribed that the limits could not be fixed with accuracy. This ecchymosis never covered the sutures, which always presented their bluish-white colour. Of the eleven who presented no ecchymosis, two were twins, two the produce of a triple birth, and the other eight were aged forty hours, a period at which the ecchymosis might have disappeared. These ecchymoses were completely circumscribed, and had their seat in the subcellular tissue, which was consequently thickened, and presented a more or less violet tint. Valleix's theory, then, is that the pressure of the child's head against the mouth of the uterus causes *cephalæmatoma*. This is strengthened by the fact of its not appearing in the case of the twins, when the mouth would be dilated on the passage of the second child. But by answering the following questions, we may explain most of the objections which may be brought against this theory.

Thus it may be asked, How is it that *cephalæmatomata* are always seated on the parietal bones? First, because it is generally admitted, that the position of the fœtus *in utero* is such, that one of the parietals is pressed against the *os uteri* more than any of the other bones of the head; Secondly, it is found that the structure and position of the bone afford every facility to extravasation.—How is it that it occurs more frequently on the right than on the left parietal? Because the right is found to present much more frequently than the left.—How is it that this disease is so very rare? Because the causes the most favourable for its production, viz. those in which a large part of the parietal, to the exclusion of the other parts of the cranium, presents itself to the *os uteri*, are of themselves very rare. This theory is, to all appearance, the one which bears the semblance of truth, and is therefore worthy of our belief, at least, until another more plausible can be given. It would go far to strengthen the opinion of Valleix, if future observation can show that they occur in cases where the first stage of labour is tedious and difficult, or where the membranes burst at an early period.

It is of the utmost consequence that we be able to distinguish *cephalæmatoma* from *hernia cerebri*. In both we have the hard projecting circular border, and the soft spongy feel. In *hernia cerebri* we have always a pulsation synchronous with the pulse, which some say they have felt in sanguineous tumours, but these instances are very rare.* The only way to account for this statement is that the individuals have not been very minute in their examination, and mistaken the pulsations of an artery running over the tumour, or in its neighbourhood, for a pulsating tumour. A similar error has led surgeons to mistake a tumour lying over a large artery for an aneurism. But there are three characters which distinguish them. The former never occurs at the parietal bone, where the latter is invariably met with, but is always situated either on one side of the fontanelle, or in the course of the sutures. *Hernia cerebri* retires and disappears upon pressure, producing vertigo and convulsions; whereas sanguineous tumours never give rise to any uneasiness, or disappear upon pressure. The supposed cases of *hernia cerebri*, which occurred on the parietal bones mentioned by Ledran,† Trew,‡ and Detharding, when attentively read, and their treatment considered, appear to have been only sanguineous tumours. They disappeared by the use of discutients. A case similar to that at page 124 could only be known as complicated with *hernia cerebri*, by the constitutional symptoms. Cephalæmatomata are distinguished from aneurism by anastomosis, and nævus, by wanting the soft feel and colour; the latter can be emptied on pressure, and on that being removed, again filled. At the same time they often enlarge upon crying or coughing. The other solid tumours in this region do not present the fluctuation which is met with in bloody tumours, and the encysted are movable, whereas the cephalæmatoma adhere closely to the bone. Abscesses of the scalp can hardly be mistaken for cephalæmatoma, as they are accompanied by pain and redness of the integuments. This, however, happened in the case of a soft, irregularly, round, fluctuating tumour, with a very distinct surrounding border, situated a little above the left parietal eminence. The medical attendant, considering it to be a sanguineous tumour, thought it improper to interfere. The child soon died, when on dissection it was found to be an abscess.* Flint has reported a very rare tumour which was situated on the occipital bone, and communicated with the sinus by an opening in the bone. This tumour was opened, and the child died from hæmorrhage.† Busch gives a similar case. As these tumours are exceedingly rare, they can hardly enter into our diagnosis, but they would be distinguished by their situation, and in that they must be affected by the circulation.

---

* Levret, Journ. de Medecine, p. 411, 1772.

† Ledran, Observations, Chirurg. Tom. i. obs. 1.

‡ Commen. Lit. Nov. p. 412, 1738.

It might be of great consequence in a medico-legal point to distinguish the cephalæmatoma, which is never the consequence of external violence, from contusion and ecchymosis, which are always so.

Prognosis and Termination.—The prognosis may in general be very favourable, unless the tumour is of great size, and has been permitted to remain undiminished for twenty or thirty days. If such a period has elapsed, it may happen that the bones have become affected; but fortunately this is rare, even in cases where the blood has remained longer than the period above-mentioned.

When they terminate fatally, it is owing either to mortification of the integuments, or to necrosis of the bone, and excessive purulent discharge.

There are two different opinions with regard to the manner in which they terminate favourably. These depend upon the fact, whether the bone or the periosteum secrete osseous matter. Valleix admits that he has perceived signs of ossification of the periosteum, which has been described by Chelius, ‡ Schmidt, and lately by Naegele. This last author describes very distinctly in a letter to M. Velpeau, the manner of the termination of these tumours. He says, after describing the nature and treatment, " it is only at the end of fifteen days or three weeks that the tumour commences to diminish. Towards the fourth week it is clearly observed that it begins to resist on pressure being made. If you apply the finger on the summit, you cause a depression, which disappears on the pressure being removed. It is exactly as if you pressed upon a roll of copper-foil or upon parchment. In proportion as the tumour hardens, it diminishes and becomes insensibly flatter.§ The following, then, are the views of Naegele with regard to the manner of cure. First, the detached pericranium ossifies on its interior surface. Secondly, in proportion as the extravasated blood is absorbed, the ossified pericranium approaches the bone, and finally unites perfectly with it. Thirdly, after six months or even a year an eminence is remarked on the spot where the tumour was seated. Fourthly, in children who have died at the end of six months or one year, M. Naegele has found by dissection that the parietal bone was much thicker at the seat of the tumour than at any other point of its extent.

---

* Valleix, Clinique des Maladies des Enfans.

† Extracted by Chelius from New English Journal of Medicine, Vol. ix. 1820, p. 112.

‡ Chelius, Traité de Chirurgie, French translation, p. 110, Tome ii.

§ Velpeau, Traité des Accouchemens, Tome ii. p. 596.

Valleix gives a different view. He says, "I have seen two cephalæmatomata terminate without operation, and the following are the results. They existedon the same infant, and, though very small, the osseous circle was very considerable. Every day this circle made new progress from the centre to the circumference, so that daily the fluctuating part of the tumour diminished. At last only a small excavated point, containing fluid, was to be felt at the top of the osseous protuberance; but this point never offered either the hardness or the crackling noise of parchment mentioned by some authors." In a word, the ossification extended from the sides to the middle, and from below upwards, that is to say, it took its origin from the bone.*

Which of these statements are correct? I would say both are equally so.

It is quite clear from numerous experiments that the periosteum does throw out osseous matter.† To support this opinion we have even the statement of M. Valleix, who, at the same time that he denies the agency of the periosteum, confesses that once he found hard gritty particles on the interior surface of the detached pericranium; and why did he not find it in all those dissections which he has so well described? Because the tumours had only existed in the one case ten, and in the other nine days, when he examined them. Here there was no time for ossification being established. When I first turned my attention to this subject, I saw a case which fully established in my mind the part which the pericranium performs. It was shown to me by the late Mr Scott, then house surgeon to the Lying-in Hospital, Edinburgh. The tumour had existed for about thirty days; ten days before Mr Scott had felt the pericranium becoming hard, and giving out when pressed the crackling sound of parchment. The day I examined it, I felt the pericranium hard and ossified, just as if a small watch-glass was contained within the membrane. At one point there was a small osseous spoke going to the bony ring, but not quite joined to it. Mr Scott described to me another case, which he had in Greenock, where the pericranium was distinctly ossified, but where the ossification commenced at the circumference, and spread to the centre. In two other cases seen by Dr Simpson the pericranium was also ossified. It being seen that the pericranium does throw out ossific matter, and finding by dissections that the bone forming the base of these tumours is also covered by rough bony granulations, we must conclude that the ossific matter is secreted both by the pericranium and by the bone;

---

* Journal Hebdomadaire, Jan. 1836, p. 27.

† See experiments by Professor Syme, Vol 14th, Trans. Royal Soc. Edin.

that these unite and form the callus, which remains often for six months or even a year after the fluid contents have been absorbed. This callus, Osiander states, must not be mistaken for a very rare congenital tumour, which consists in an hypertrophy of the diploe of the parietal or other cranial bones, as, if the latter be opened, death is a certain consequence.*

Treatment.—Various means have been devised to procure resolution of these tumours. Moscati and Palletta employed the seton; a dangerous practice; and they confess that their patients frequently sunk under the acute symptomatic fever which was caused by its use. Gölis used caustic potash; but this harsh treatment was often followed by necrosis of the bone. Nestor, after fifty years' experience, says, " Perforare iteratò tumorem, vel setaceo trajicere salubre est; prae omnibus autem praestat aromatiis resolventibus sive fomentis sive cataplasmatibus sanguinem discutere."

Discutient applications combined with slight pressure are decidedly the best applications, especially if the tumour is small; but whenever it threatens to inflame, it ought to be opened without delay. The incision should be small, and kept open for a day or two by the introduction of a small piece of lint. Zeller gives three cases showing the comparative efficacy of discutient applications with incisions. A female child, which was born with ease, had on both parietal bones a sanguineous tumour, almost equal in size. That on the left was opened, and at the end of eight days was cured. The other disappeared in five weeks under the use of discutient lotions. In another case of a male child, where the tumour was larger, and covered the superior and anterior quarter of the right parietal, it disappeared after incision in less time than the former case, and in a still shorter period in a third case.† Incision or rather puncture may be employed in cases where the tumour is very large, and where the parents of the child become anxious for its disappearance. A case is mentioned of a cephalæmatoma disappearing after epistaxis; but as this was reported to the medical man (who had not visited his patient for some time), and not seen by him, we can place no faith in the accuracy of the statement.‡

There is little chance of hæmorrhage following the incision. Smellie, however, mentions that one of his pupils having opened one of these tumours, hæmorrhage occurred, which could not be restrained, and the infant sunk in a very short time. Valleix§ gives a case where he opened the tumour, and a great quantity of dark blood flowed, and then a small quantity of arterial. " I examined with care the parts, but the hæmorrhage having ceased, I only used gentle pressure. The day after this the child died, and I saw that the compresses were deeply stained with blood, and the most dependent part of the tumour contained a small quantity. On dissection I found that I had cut across a branch of the posterior temporal artery."

---

* Chelius, Traité de Chirurgie, French translation, Tome ii. p. 110.

† Journ. Complementaire du Diction. des Sciences, Med. lxiii. Sept. 1822.

‡ Presse Medicale, No. 54, 1837.

§ Collection of Extraordinary Cases, Paris, 1770.

Third Class. Subcranial.—In this variety the blood is extravasated between the *dura mater* and the cranium; an accident which Velpeau says is by no means uncommon during delivery. This, which by some is called internal cephalæmatoma, is not confined to the parietal bone, but is met most frequently in the occipital region. Baron states that, in the few cases which he has met with of internal cephalæmatoma, he has always found an external one.* In the first stage of this tumour, there is sometimes constitutional symptoms, such as twitchings of the limbs, stupor, &c.; but more often the disease proceeds insidiously, either producing inflammation of the membranes, or destruction of the bones, and only then is constitutional disturbance produced. Hence, when there is no external appearances, the diagnosis is almost impossible.

Hoere describes a case in which there was a sanguineous tumour between the *dura mater* and the bone, and a second between the pericranium and the bone. These two communicated by a loss of substance of the bone (fissure.) The internal tumour was of the size of an egg, and caused a depression in the corresponding part of the brain. The external plate had suffered no alteration; the internal, on the contrary, was carious, and even completely deficient in one point.†

An almost similar case, but complicated with *hernia cerebri*, is reported by M. Brouisseaux. A healthy young woman gave birth to an infant with a tumour upon its occiput as large as its head. The developement of the tumour was attributed to a contusion received by its mother in the first months of pregnancy. The child lived fourteen hours in a state of almost absolute insensibility. All the muscles depending upon cerebro-spinal influence were paralysed; suckling was impossible; and deglutition could hardly be performed when milk was poured into the mouth. The alvine evacuations took place naturally; the eyelids were constantly closed, and the ball of the eye remained immoveable; the pulse could be felt, though very feeble; the motion of the thorax in respiration very slight. The tumour, which was nearly round, soft, opaque, and ulcerated in many points, presented a neck one inch in length, and three in circumference. On grasping it, especially towards the cranium, a greater resistance was felt than in any other part. The child died; when, on opening the tumour, it discharged about four ounces of corrupted blood. On extending the opening, a tumour of a lively red was perceived, of the size of a turkey's egg; the opening of which gave exit to a considerable quantity of very thick black blood, and to three clots of the size of a hazel-nut. The second tumour communicated with the brain, through an opening situated behind the *foramen magnum*. It gave passage to a portion of the *medulla ob-*

---

* Diction. des Sciences Medicales, Art. Cephalæmatome.

† De Tumore Cranii recens natorum sanguineo, &c.

longata, six lines in length, which presented no signs of alteration, either in colour or structure.

Treatment.—I can say little on the treatment of this variety, as I have been unable to procure any cases in which remedial measures have been used. But, considering the severity of the lesion, and the delicate age of the patient, we cannot expect that art can be of any avail, either in evacuating or causing absorption of the contents.

ART. XIV.—*Two Cases of Rupture of the Carotid Artery from Sphacelus.* In a Letter to the Editor. By Mr C. J. MILL, Surgeon, Kirriemuir.

Two cases of rupture of the carotid artery from sphacelus, followed by fatal hemorrhage, have happened in my practice during the last six months. Both resulted from scarlatina, which had no medical treatment during the first stage. As it is of rare occurrence, and little noticed in medical writings, I transmit you a short account of them.

CASE I.—J. M. aged 3 years, stout and full. At my first visit the disease had continued about six days. The rash was very prominent over all the body; almost copper-coloured, and with vesicles interspersed here and there. The fever was very intense; the pulse full and rapid, as might be expected; for the patient had been kept as hot as possible, and stimulants given " to put out the rash," as the parents said. The throat from the first had been much affected, almost preventing deglutition and articulation. The glands in the neck were swollen and painful to the touch. I immediately applied leeches, warm fomentations, and a blister to the throat. Nauseating diaphoretics, given so as to cause vomiting occasionally, purgatives, cool temperature, tepid bathing, light diet, &c. formed the rest of the treatment. By this the patient was much relieved, and in a few days could sit up a little. But a fresh accession of fever came on, and the throat became rapidly worse. I could never get a view of the pharynx, owing to the swollen glands preventing the opening of the mouth. Leeches were again ordered, and warm poultices every second hour. Matter soon began to point over the submaxillary gland. I made a puncture, and evacuated about four ounces of healthy purulent matter. As the pulse was now weak, I ordered animal jellies and wine to support the strength. The patient again rallied, and was able to sit up a little in bed several times a day. The discharge from the opening in the neck continued, the fever declined gradually, and sleep returned, until the sixth day after opening the abscess, when an unfavourable change again happened. The pulse became quick, weak, and irregular. The patient lay on his back with a clammy cold and pale countenance, the features having that expression called hippocratic. Involuntary dejections, &c. foretold a fatal termination soon. I mentioned to the friends that the child was fast sinking. On the evening of that day a sudden and most violent discharge of arterial blood took place from the mouth and wound in the neck. In the course of three minutes, about a chopin had escaped. By the time I reached the house the child was dead. A large tumour extended from the right ear, as far as the clavicle, caused by the accumulation of blood below the fascia.

Examination of the parts was not allowed.

CASE II.—I was called to Ann ——— aged 7 years, about the tenth day of the fever, which was still intense. The throat had been and still was severely affected; but the eruption was completely gone. The pulse though rapid was weak. I did not consider leeching proper, but applied a blister and hot poultices. Afterwards gentle laxatives, nourishing diet and wine were given. The symptoms continued much the same for eight days. On the evening of the 18th day from the date of seizure I saw the child, and told that death would soon happen. In about two hours I was sent for owing to a most profuse discharge of blood from the mouth having occurred. Before I reached the house the child was dead.

No examination was allowed.

In such cases as these nothing can be done. For it is owing to the disease being neglected in its first stage, that it is so severe afterwards. The practitioner ought to bear in mind the possibility of such an occurrence in cases of neglected pharyngeal inflammation; though fortunately they are rare.

I think that the inflammation in both cases had commenced in the mucous membrane covering the pharynx, and, being aggravated by maltreatment, had terminated in sphacelus; and that it had spread from the mucous membrane of the throat to the tonsil and cellular tissue and sheath surrounding the blood-vessels. My reason for this opinion is, that in scarlatina the mucous membrane of the throat is always more or less primarily inflamed; and in many cases the inflammatory action went on steadily increasing, until the fatal termination. The constitutional symptoms already given indicated mortification. The blood was decidedly arterial. It came so rapidly and suddenly, as clearly to indicate the perforation of a large blood-vessel. From all these reasons, therefore, I considered myself perfectly warranted in terming the cases sloughing of the artery. Dissection would have set this question completely at rest; but to that, popular prejudice in this northern quarter forms as yet an almost insuperable barrier.

ART. XV.—*Cases illustrative of the Division of Tendons.* By WILLIAM RHIND, Surgeon, Edinburgh.

THE division of tendons in various parts of the body, first performed by Stromeyer and Dieffenbach, and, especially in cases of Talipes, introduced to the notice of the profession in Britain by the excellent work of Dr Little, still continuing to excite much inte-

Edinr. Med. & Surg. Jour.

Mr RHIND'S CASES OF DIVISION OF TENDONS.

Plate VII, p. 126, Vol.

Fig. 1.

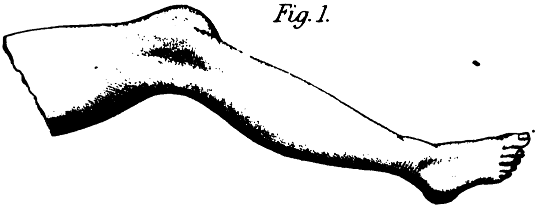


Fig. 2.

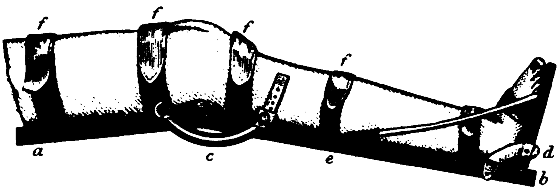


Fig. 3. Fig. 4. Fig. 5. Fig. 6.

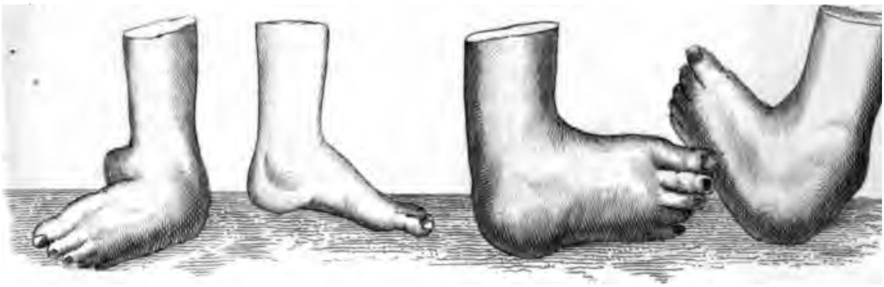


Fig. 7.

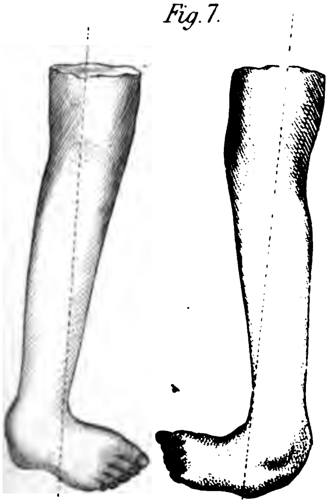


Fig. 8.

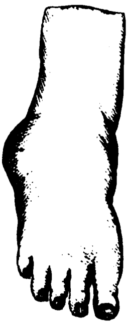
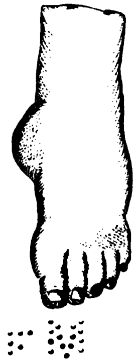


Fig. 9.



UofM

rest among medical practitioners, has induced me to offer a few cases in illustration of this most useful invention, in the hope that the detail of practical results may still be a desideratum with the profession.

Case I.—*Division of the Tendons of the Ham for cure of contracted Knee-joint.*—Miss F. aged 30, when about 8 years of age, fell and received a contusion in the right knee, suppuration ensued, and a tedious ulceration and discharge continued for several years. On the ultimate healing of the ulcer, partly, perhaps, from the effects of bandaging the joint, and partly from drawing up the leg to relieve the uneasiness of walking, it was found that the knee-joint had contracted considerably, and in this state it has remained till the present time. Her general health is now good, but in walking she can only put the point of the great toe of the affected leg to the ground, while the heel is elevated four inches. In consequence of this she walks with great difficulty, and with the appearance of great lameness. On examination I found the tendon of the biceps muscle very tense, as also the tendons of the semi-membranosus, semi-tendinosus and gracilis, though in a less degree. On attempting to press the foot to the ground, the *tendo Achillis* seemed also very tense, although on examining the foot in the relaxed state there was free motion in the ankle-joint, and no deformity or undue rotation of the foot. On further minute inspection of the ham, I perceived the tendon of the plantar muscle somewhat contracted; and this I concluded was the cause of the tension behind the ankle when the leg was extended in the erect position. Fig. 1, Plate VII., represents the extent of contraction of the knee-joint. Finding that there was full motion of the joint within the limits of the contracted tendons, and that the surrounding parts were in a perfectly healthy state, I at once proposed an operation, which was agreed to. On 3d October 1840, I divided the tendon of the biceps by introducing a small bistoury under the skin, a little to the outside of and above the tendon, and, turning round the edge of the knife, I cut it through at two strokes from above downwards. I then introduced the bistoury on the inside of the ham, and divided the tendons of the semi-membranosus and semi-tendinosus and gracilis muscles in the same manner, and with the same external opening, turning the knife towards the middle of the ham, I raised up by extension and carefully divided the plantar tendon. It was my intention also to divide the *tendo Achillis* at the ankle, if I found that the division of the plantar tendon had not relieved the tension of the former; but on examination, I found that this had completely effected the object desired. There was only a few drops of blood flowed from the first puncture, and a slight venous hemorrhage from the second. A compress and bandage were applied, and the whole limb was bandaged up and retained in its original bent position by means of appropriate splints.

7th, the patient has passed two quiet days and nights without any pain or uneasiness. To-day a splint was put under the limb, and the bandages partly removed, gradual extension was employed, and the heel easily came down one inch, when the apparatus was adjusted so as to prevent farther extension.

9th, Slight pain last night around the upper part of knee-joint, and slight spasm of muscles. Removed the dressings to day. The outer puncture is completely healed up; slight sanious discharge from inner wound. Extension to-day of about half an inch; no pain or uneasiness. A stimulating lotion ordered if the pain and spasm return.

10th, A good night, and no return of pain; extension gradually continued.

16th, Extension of knee now complete, and the heel on approximating it to the other is one-fourth of an inch shorter. This arises from the developement of the bent leg having been thus much retarded. There is a slight ecchymosis on the inside of the ham, and a small discharge of venous blood from the puncture, for which a stimulating lotion was ordered.

Nov. 7th, A gradual improvement has daily taken place; the wound inside the leg is now healed up, and all the divided tendons feel strong and continuous. There is a complete motion of the knee-joint without pain, and when the foot is placed on the ground, the heel is on a level with the toes. The patient can walk a little with ease, placing the heel and sole completely on the ground; and it is evident that a little more strength and confidence will enable her to walk without pain or halting. The tepid salt water bath and friction of the whole leg and foot ordered to be persevered in.

As the object in this and similar cases is to conduct the extension in a gradual manner, so as that the tendons in their process of union may be elongated to the extent required,—and this may, in some cases, be not less than two to three inches, according to the degree of curvature,—an apparatus constructed for this purpose is represented in Plate VII., Fig. 2. *a b* is a strong wooden splint hollowed out in the middle and padded, with a hinge at *c* and a sole-piece at *b*, also hinged with its axis at *d*, corresponding to the axis of the ankle-joint. A strap with a buckle on each side, as seen at *e*, will serve to make the necessary flexure of the foot and ankle-joint, so as to prevent any chance of contraction during the process of cure, or serve for extension in cases where the *tendo Achillis* requires division. The semicircular metallic plate at *c*, passing through a staple, may be secured at any point by means of a common nut screw, and will thus enable the knee-joint to be gradually and cautiously extended. Whenever the extension becomes painful this joint may be slackened as well as the straps and bandages attached to the splint, as seen at *f f*. I have not seen the instrument used by Stromeyer in similar cases, nor a description of it, and therefore cannot say how far it coincides with the above; but this was the first obvious suggestion that occurred to me, and it is sufficiently simple and suitable for general use.

I have selected the following cases of talipes from a considerable number lately treated, because they are of an extreme grade, in order to show the success of such operation: even under unpromising circumstances.

Fig. 3, Plate VII. represents the left foot of a boy aged 9. Congenital varus where there was extreme contraction of the tendons and an angular flexure of the tarsal bones, aggravated by the injudicious application of a steel boot, some years before. The cuboid bone projected considerably from its place.

August 12, 1840.—In this case the *tendo Achillis*, the *tibialis anticus* and *posticus*, and the *abductor pollicis* were divided. In a few days, by means of the usual extension, the foot was brought nearly straight; but it was now discovered that the *plantar fascia* in the sole, and the *flexor pollicis* were also much contracted. These were immediately divided, and pressure applied to the projecting tarsal bones, by means of a pad and bandages. September 10, The patient can now put the sole to the ground, and walk with considerable ease; but there is still a stiffness of the ankle-joint and a partial displacement of the cuboid bone.

By means of a pad and bandaging and assiduous friction, the foot gradually assumed its natural position, and it now presents the appearance as represented by Fig. 4.

The boy can walk with ease, and merely requires a boot with a high heel, as the lame foot is about half an inch shorter than the other. The retardation of the cure here was the displacement of the tarsal bones, and the extreme contraction of the *plantar fascia.*

Fig. 5, 6, represent the right and left feet of a female aged 25, with congenital varus. In the right foot the heel is very little elevated, and there is but slight contraction of the *tendo Achillis*; but the other tendons of the inner ankle are very tense and unyielding, and bend the tarsus and toes at nearly a right angle inwards. The left heel is much more elevated, and the foot and toes more twisted inwards and upwards from the rigid contraction of the *tibialis anticus* and *posticus* and the flexors of the toes. There is also in both, but especially in the left foot, a considerable separation of the bones of the tarsus and metatarsus on the outer side, and a great thickening and enlargement of the integuments and bursæ, forming a large artificial pad or heel which, in the left especially, often becomes inflamed and painful if much exercise is taken. The whole left leg from the knee downwards has also a considerable twist inwards, and to a much greater extent than the right leg.

September 8, 1840.—In presence of Dr Abercrombie and Sir George Ballingall, I divided, in the right foot, the *tendo Achillis, tibialis anticus*, and *plantar fascia* with the flexors of the toes; in the left, the *tendo Achillis, tibialis anticus*, and *posticus, abductor pollicis*, and flexors of the toes.

11th, There has been no uneasiness, and the wounds are all healed up. Extension was begun to day. In consequence of the extreme curvature of the feet, the usual instruments of extension could not be applied. I had therefore prepared two long splints with hooks at their extremities, which were applied to the fibular sides of each leg, interposing an air cushion between each. In this way considerable extension was made and persevered in for a few days, until first a modification of the Scarpa shoe was applied, and at last the Scarpa shoes themselves.

October 2d. Considerable progress has been made by using the Scarpa shoes during the day, and night shoes of a simpler construction, with occasionally the use of the long splints and other apparatus.

On examining the right foot more particularly, I found that the undivided *tibialis posticus* is one cause of obstruction to its full extension, divided it to-day about half an inch from its insertion in the *os navicularis.*

16th. Extension of right foot has greatly advanced. In the shoe it appears almost straight, and the sole and heel come down with ease. The patient can walk about tolerably well. The left foot is still considerably curved with great tendency of the whole leg from the knee to turn inwards. This has to be obviated by a splint, which reaches from the knee to the toes, resting upon the edge of the Scarpa shoe, and which is bandaged tightly along the whole leg. Additional compression is also applied to the left foot. The diligent use of fomentations and liniments, especially over the thickened integuments, is also enjoined. The old heels have now become soft, and are rapidly undergoing absorption.

November 4th. A pair of common boots have been worn for some days. The right foot is almost perfectly straight with the slight restraint of the boot. The left has still a tendency to turn inwards, but the heel and sole now come to the ground, and the motion of the ankle-joint is greatly more free.

9th. The patient now walks a little through the room; the artificial heels are fast disappearing, and thus the feet are assuming more of the natural symmetry. The fore part of the foot and toes can now be easily kept in a straight position by the simple restraint of a laced boot, but the opposing ligaments and muscles on the outside of the foot and leg have not yet so contracted as to resume their power to retain the foot in its straight position without assistance. Indeed, in consequence of the great relaxation of the peronei muscles and tendons, as also of the overstretched ligaments of the outer edge of the tarsus, a considerable time must elapse before the proper action of the fore part of the foot is acquired. In the meantime, however, there is a firm footing of the heel, and a motion of the ankle-joint never before enjoyed.

Fig. 8 and 9 exhibit the shape and position of the feet at this period.

In this case I have already alluded to the inward twist of the leg from the knee downwards. There is an unusual looseness of the articulation of the upper end of the fibula with the tibia, as well as a corresponding looseness and deficiency in size at the inner ankle of the *malleolus internus.*

These arise from the constrained and unnatural mode of progression hitherto practised, in consequence of the uneven surface which the side of the foot presents in walking, and its great tendency to inversion, aided also by the contracted state of the muscles of the inner side of the leg, which have a constant tendency to pull the tibia and fibula inwards.

Fig. 7. The case of a boy, aged 4½ years, with congenital varus of both feet, is given to illustrate this twist; the dotted line *a a* marking out the natural position of the femur and tibia. In this case the cure of the varus, in so far as the feet were affected, was almost completed in two weeks, yet much subsequent pains were requisite to overcome the twist in the legs, and to bring back the muscles to their legitimate action. For this purpose long splints and bandagings were used, and when at rest placing the knees and legs parallel to each other, and drawing them into contact by belts or bandages.

The efficacy of the division of tendons is no less obvious, even in incipient affections of the muscles not congenital, as the following case illustrates.

D. A. aged 4 years, had long been observed to have a weakness of the right ankle. He often fell when running or even walking, and his foot had a tendency to turn outwards, forming a slight *talipes valgus.* On examining the foot I found the peronei tendons tense, and apparently a want of action in the muscles of the inner side of the leg and ankle. The tendons of the *peroneus longus* and *brevis* were divided, and the foot so bandaged as to incline the toes inwards. This was continued for several days till the divided tendons reunited, while a stimulating embrocation was rubbed on the inner side of the leg and ankle. Ten days after the operation the toes in walking had now rather a tendency to fall downwards, but by adjusting a piece of whalebone on the boot, so as to give a support on the outer ankle, and fixing a strap to prevent the extreme flexure of the joint, the step in a few days became much firmer, and he evidently walked with more facility, with less inclination to turn out the toe. In two weeks he returned home, and his father writes me that "he has improved much in walking, and now very rarely falls." In course of time there is little doubt but this would have turned out a confirmed valgus.

I conclude with a few general deductions from facts.

With regard to the mode of operating, I generally prefer cutting the tendon from without inwards, instead of slipping the knife below the tendon, and cutting outwards, as recommended by Dr Little. By making the proper extension, the contracted tendon can always be raised sufficiently, so as not to endanger any of the neighbouring vessels; and a little experience will enable the operator at once to distinguish, by the feeling communicated through the knife, when the tendon has been completely severed, and thus he will be prevented from going deeper.

When neatly performed, the operation causes very little pain; indeed the whole pain consists in penetrating through the external skin. The severing of the tendons may be called a peculiar uneasy feeling rather than decided pain.

In no instance have I known or heard of any unpleasant effects which followed the division of tendons; on the contrary, adhesion of the skin, with very few exceptions, takes place in two or three days, and a junction of the divided tendon in a few days more. In one or two cases, from the extreme restlessness of children, slight suppuration of the external wound occurred; but this had no injurious effect on the tendon beneath. In two or three cases the adhesion of the divided tendons was so complete in four days as to admit of motion of the corresponding muscles. In other cases, the callus has remained in a soft and relaxed state for weeks. These are circumstances to be carefully looked after by the practitioner, otherwise the success of an operation may be frustrated, either by undue delay in making the necessary extension, or by premature extension endangering the total separation of the divided ends of tendons. It may be satisfactory, however, to know that a divided tendon, whose extremities have been separated for a whole month, will unite by simple apposition of the divided ends. Four years ago a lady applied to me with her middle-finger completely bent inwards, in consequence of a cut with a sharp knife, which she received a month before on the back of the middle joint of the finger. On examination, I found that the wound was completely cicatrized, and the ends of the divided tendon were distinctly felt on each side of the joint entirely apart. The separation had existed, as I have said, for a month. I bandaged up the finger with a splint inside, to keep the two ends in contact, and enjoined her to preserve it in this position for at least a month, with, I confess at that time, but faint hopes of success. Within the month, however, I had the satisfaction to find that union had taken place, and that the full use of the joint was restored.

It is truly astonishing also to witness to what an extent a severed tendon will elongate in the course of a few days. In some cases, the amount of elongation could not be less than from 2½ to 3 inches in little more than ten days. The absorption of the false heel is also a matter of interest to the surgeon, and often of anxiety to the patient. The persevering use of frictions and well adjusted pressure are the best means for accomplishing this. I have found an ointment of hydriodate of potass and a few grains of iodine most useful for promoting this process. In some cases these callosities become quite soft and greatly distended with a fluid, a short time previous to their final disappearance. This fluid, I presume, is the synovial liquid, proper to the joints of the foot.

In the great majority of cases which have come under my own observation, the right foot is most frequently affected, and when both feet are deformed, the right is generally more contracted than the left. This fact was first called to my notice by my friend Mr Braid of Manchester, who remarked the same in the extraordinary number of cases which are passing so successfully under his hands.[*] On reference to Dr Little's book, the same remark holds with regard to his cases therein enumerated. Others have, however, had different results. (See Dr Thomson's table in Med. Journal, No. 144.)

The contraction of the foot retards its due developement, especially its elongation. All the talipes feet I have seen are shorter than the natural and average size, and in single cases the affected leg is uniformly shorter than the other. After cure, however, especially when the heel has been much drawn upwards and inwards, there is an addition to the length of the leg, sometimes amounting to an inch.

In one case I found *talipes varus* hereditary. In another instance varus occurred in a nervous child when two years old, whose father was affected with nervous convulsions, in consequence of intemperance.

In a patient with varus of the left foot, there was a tendency to valgus of the right. The varus was cured by operation, and the valgus benefited by a properly constructed boot.

In young subjects, the cure is of course more expeditious and more perfect than in cases far advanced and aggravated by much walking. Congenital cases seem to be more severe and more difficult of cure than non-congenital.

There seems to be no limits, however, with regard to age, provided the period is within that in which the renovating powers of the constitution are still vigorous. Nor are even the most unpromising distortions incapable of relief and even complete cure. Dr Little has justly remarked, that in this department of the healing art, we have to wage a continued conflict with nature. It is wonderful how much perseverance will effect. The ligaments, and tendons, and bones, which seem at first as rigid and inflexible as the trunk of the oak, begin at last to give way. They first become yielding and elastic, and at length assume all the suppleness of their original nature. We have already alluded to the absorption of the indurated integuments and cellular membrane. Even the raised and thickened edges of bones that have been thus enlarged by undue pressure, after a time become smoothed down by the accommodating activity of the absorbents.

The last matter for consideration is, what are the class of cases in which the operation of division of the tendons is to be recommended, and what are those in which the beneficial results of an operation are more doubtful?

The operation is so perfectly safe, and infringes so slightly on the constitution, that in all cases of young and middle-aged persons, where there are prospects of ultimate advantage, there ought to be no hesitation in putting it in practice. In the case of contracted knee-joints, for instance, where the leg is generally useless, and sometimes an incumbrance, the probability even of success would be sufficient to hazard such a harmless attempt at restoration.

---

[*] To this enthusiastic practitioner of the Stromeyerian operation, I owe many valuable practical hints, as well as much personal and professional kindness and liberality. It is to be hoped that the public will soon be put in possession of the results of his ample experience. On this account, I forbear touching on some new and interesting points connected with the subject, which he has been the means of eliciting.

It is the same in all those cases which may be called half or incomplete varus, where the foot is not completely turned round and fixed in an uniform position; but where it turns inwards and on its edge every time the patient treads on the ground, this with little apparent distortion causes great lameness and almost total incapacity of locomotion. In all these the cure is easy, speedy, and most complete. In old confirmed cases of varus, on the other hand, there may be some grounds for hesitation, especially in cases of double varus of the extreme grade. In these, long habit has enabled the sufferer to form an artificial sole and heel, on which he walks with comparative facility, although with a great and laborious expenditure of his whole muscular system. It requires the action of almost all the muscles of the trunk and extremities to preserve a centre of gravity so different from that of ordinary men. In such cases the cure is comparatively much more difficult and protracted. It is a work of time and incessant perseverance, and requires all the assiduity and tact both of the patient and medical practitioner,—a mind fertile in resources, and a hand accustomed to mechanical manipulation. Without all these, sad failure will be but too often the sequel of all such attempts. Time, means, opportunity, station in life, and even cast of mind and feelings, must all form elements in determining cases of this class.

Attached to deformity there is a feeling which nothing but personal experience can communicate,—a depressing idea of inferiority and of difference from one's fellows,—a sensitiveness to the contempt of the unfeeling and rude, or even to the careless curiosity and wonder of the thoughtless,—a shrinking from the gaze even of one's friends,—and a feeling ever recurring to the mind that the peculiar physical infirmities are the objects of remark, when no such observance, perhaps, ever occupies the thoughts of those around. All these, in spite of philosophy, and even under the discipline of a more sacred and self-humbling creed, prey upon the mind,—cast a chill over the glowing blossoms of youth,—interfere even with the sobered thoughts of maturer years,—influence the actions, and determine the very cast and tenor of the way of life. This feeling, perhaps, had its share in setting the too sensitive Byron at enmity with his kind,—added deeper cunning and finesse to the subtle mind of a Talleyrand,—restrained Scott from the Court, and "tented field," and goaded him on to all manner of ambitious shifts of establishing a name and family, to make up for the consciousness of physical infirmities. The hump-back, no doubt, added an additional sting to the satire of Pope, as similar deformities have soured the otherwise bland tempers of many men of inferior mental capacity. It is this feeling which renders young and old so patient under the process of cure; so full of hope and anticipation, and so willing to undergo physical pain and temporary confinement. While other patients are depressed with the disappointments and unexpected afflictions of disease' these are full of buoyant spirits, and patiently endure, without any encroachments on their general health, restraint and uneasiness, which, under other circumstances, would be all but intolerable.

It is this feeling which arouses the intense interest of the mother hanging over her deformed child, when accounts of cure and restoration reach her ears; and when at length the shapeless and contracted mass of limb begins to assume its natural symmetry, and at last receives the handsome boot, exhibiting all the proportion and lineaments of a symmetrical form, one can scarcely say whether the countenance of mother or child evince more heartfelt exultation and delight.

8, *Hart Street*, 12*th November* 1840.

Art. XVI.—*Appendix to Dr J. Reid's Paper on the Anatomical Relations of the Blood-vessels of the Mother to those of the Fœtus.* (Vide p. 1.)

I have had to-day an opportunity of examining an impregnated uterus, through the kindness of the Curators of the Museum of the Royal College of Surgeons here, and I beg to add the results of that examination as an appendix to my paper on the Relations of the Blood-vessels of the Mother and Fœtus. It was apparently between the fourth and fifth months of pregnancy, and had been preserved for a long time in spirits. It had been cut open in front to show the interior, and in doing this a section had been made of that part of the uterus to which the placenta adhered. To avoid injuring the preparation, I examined only it along the cut edge. I found that an injection had been thrown into the uterine arteries, which only partially filled them. The veins were not injected. Several of the *curling arteries* were well filled with injection, while others contained only a small quantity. On tracing these from the inner surface of the uterus through the decidua, they were observed to terminate among the tufts of the placenta, without undergoing any change in their calibre during their oblique course through the decidua. The injection filled up the intervals between the tufts of the placenta in the immediate neighbourhood of the point of termination of each. These curling arteries were not continued into the placenta in the form of a tube or dilated vessel, as has been represented; but, from the manner in which the tufts adhered to their edges, we believe that the inner coat was reflected upon them, as we have already stated. On examining the utero-placental veins, the tufts of the placenta were in many cases observed to project into their open mouths, and to adhere to their inner surface, but in none that were examined did they project so far as the uterine sinuses. It is possible that if the dissection could have been carried farther, that they might have been found to project along the veins to a greater extent at some other points. In the uterus in my own possession, the tufts in numerous cases projected only into the mouths of the utero-placental veins, while in many others they extended themselves as far as the mouths of the uterine sinuses, and even projected to a considerable distance into their interior. In all these different relations of the placental tufts to the utero-placental veins, the inner coat of the vascular system of the mother is, as far as I have observed, reflected upon the outer surface of these tufts. It is very probable that the placental tufts may project into the utero-placental veins to different distances in different cases, and it may be to a greater extent in the advanced periods of utero-gestation, than at an earlier period. I am the more inclined to adopt this opinion, as it has been suggested to me, not only by the result of this examination, but also by a communication from Dr Sharpey. However this may be, it does not affect the views which I have advanced regarding the structure of the placenta, for it is apparent that these do not necessarily require that the tufts of the placenta should project into the uterine sinuses. The great extent to which these tufts projected into the uterine sinuses in the impregnated uterus in my possession was chiefly of advantage in enabling me to examine, under very favourable circumstances, the mode in which the placental vessels terminated, the reflection of the inner coat of the vascular system upon the outer surface of these vessels, and in furnishing a miniature representation of the structure of the interior of the placenta.

Royal Infirmary,
26th November 1840.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Pharmacopœia Castrensis Ruthenica.* Auctore Jacobo Wylie, Equite Baronetto, S. I. M. ab Intimis Consiliario, Supremo Rei Medico-Chirurgicæ Castrensis Inspectore, &c. Editio 4ta. 8vo. Petropoli, 1840. Pp. 820.

The Russian Military Phramacopœia. By Sir James Wylie, Baronet, &c. 4th edition, 8vo. Petersburgh, 1840. Pp. 820.

Few of our readers, especially those in any way connected with the military service, would be prepared to encounter, under the title of *Pharmacopœia Castrensis*, a portly 8vo of 820 pages. Most of them will be ready to conclude from its bulk, that the nature of the work must be little in correspondence with its title, for the medical wants of soldiers are comparatively few, and the treatment of their diseases is in general conducted on the least complicated and most simple therapeutical systems. It will be necessary, in order to remove this unfavourable impression, to offer a few preliminary remarks on the origin and nature of the present work.

The name, we admit is not happily choosen. It is not, in fact, a Pharmacopœia, as that word is and ought to be understood. It is rather what is called a Dispensatory or a Pharmacologia, and its general value may be most readily comprehended, when we say that, in plan and character, it resembles a good deal the well-known Dispensatory of Dr Andrew Duncan Junior. The work is, we understand, chiefly intended as a text-book of Pharmacology for the students of the Academy of Medicine and Surgery of St Petersburgh, of which Sir James Wylie is President. The students of this academy are in a great measure young men from the provinces, who intend to serve in the army, or to practise in the districts from which they come. They receive a complete professional education at the Academy, and on examination obtain a license to practise, being ranked, we understand, in three classes according to the amount of their proficiency. It was in a great measure to serve as a compendium of Pharmacology for those young practitioners who, either in the army or in the provinces, have little access to stores of medical literature, that Sir James, twenty-three years ago, first published this work, which has now gone to a fourth edition ; and he has called it ***Castrensis***, we presume, from its being in this view a convenience for the medical department of the army of which Sir James is chief Inspector. Moreover, the title thus adopted serves (we know not whether intentionally or not) to distinguish the present work from the Pharmacopœia Rossica, which is issued under the authority of the College of Medicine of St Petersburgh.

We would take this opportunity of remarking, that, among the native Russians, medical science does not appear to be in a very advanced state, although they have good institutions for professional education, both at St Petersburgh, Moscow, Wilna, and Dorpat. The chief cultivators of chemistry in Russia are Germans. The most distinguished of their practitioners, and those who appear to engross most of the important medical appointments, are natives either of Germany or of Great Britain, and we seldom hear of any Russian contributing to medical science or enriching medical literature. We cannot suppose that this is owing to want of talent among the Muscovites. It does not appear to be owing to the want of medical educational institutions. It must, we apprehend, be due to a cause, which we are informed from a source on which we can rely, is felt to operate extensively in retarding the progress and diffusion of science in Russia,—we mean the want of good preliminary education. This is a lesson which ought not to be lost upon our legislators and medical corporations, at a time when medical reform is in contemplation.

Amongst the natives of our own country who have risen to eminence in Russia, Sir James Wylie is one of the most remarkable. Raised by his own industry and talent, to occupy the distinguished position of head of the medical department of the army, and other important offices, he has obtained an extent of influence very unusual, and which could be obtained only in a country like Russia, where good medical knowledge and general information are not very widely diffused among the native practitioners. This influence he has applied to the best purposes, and, like our own excellent Director-General, Sir James M'Grigor, he has greatly raised the character of the service over which he presides since he undertook its direction.

His zeal for his profession is no way more strongly manifested, than in the labour and care which he has displayed in the original compilation and subsequent enlargement, improvement, and rectification of the present work. It does not pretend to originality as a treatise on Materia Medica, but it bears marks of most laborious research and careful reflection, and evinces on the part of the author a desire to amass and render available, all that can in any way be valuable or useful. It is written in Latin, and is composed in an elegant style, and with as much attention to classical purity as the modernisms of the subject will admit of. Farther, we must add, before making a few remarks on some of its details, that the work is published in a style which reflects the highest credit on the Petropolitan press. We have seen few specimens of typography in this country, and none from the continent, so massive, clear, and elegant. To our taste it is too good both in size of type and thickness of paper. Had it been less so, a good deal of bulk and weight might have been saved.

The first portion of the work on which we have to offer any remarks, is that sentence in the preface which refers to nomenclature. Sir James seems not exactly at ease on this subject; for he takes out a kind of caveat against the objections which he anticipates to be offered to his changes of names. We confess we are not surprised at this. We only wonder that the good sense of the author should not have led him to avoid the error, instead of adopting, as he has done, to the fullest extent the ideas of the London College of Physicians on this subject. It is the old story of the fear of producing a patch-work, (*v.* Preface to the London Pharmacopœia), a dread of horrifying minds of sensitive or rather fastidious classical refinement, by a mixture of new and old names, the force of which we have never been able to perceive. It is quite right, that, in a work of this kind, the modern nomenclatural improvements in botany and chemistry should be fully recognized; but they should, in a practical work like the present, occupy only the second place as synonyms; the distinguishing name ought always to be something which, whether new or old, is placed beyond the fluctuations of theory. It is not to the mere novelty of the names that we object so much. We see no great difficulty in any one, especially a student, committing these to memory. What we find fault with is, that these names, being constructed on the unstable basis of theory, the effect will be, that, before the students, for whom this book has been prepared, have been ten years in practice, a new edition of this excellent work may be required, chemists and botanists will have propounded new theories, and put forth new names in accordance with them, and the student of the present day, then exalted into a military surgeon or country doctor, will have the whole task of nomenclature to study again.

Moreover, we respectfully ask on what authority are these names founded? We contend that in some instances they are wrong, being formed upon unsubstantiated hypotheses. For example, Sir James has thought proper to adopt for nitro-muriatic acid, the term *Acidum Hyponitricum Chloratum*, *i. e.* chlorated hyponitrous acid. No doubt this expresses the essential part of the nature of this mixture, its containing evolved chlorine. But we are not prepared to admit that the nitric acid has passed into the state of *hyponitrous* acid, even when, as in the present formula, three measures of *muriatic* to one of *nitric acid* are employed.

The existence of *hyponitrous* acid at all is denied by no less an authority than Berzelius, and, even admitting its existence, which we most readily do, there is no decisive proof of its being formed in the present instance. On the contrary, we infer, from some experiments of Sir Humphry Davy, when he mixed *nitrous acid* and *muriatic acid* without forming *aqua regia*, and where the formation of *hyponitrous acid* would be more probable, that such a change does not in reality take place. In what respect, then, is the theory espoused by Sir James so decidedly superior that it should be adopted as the foundation on which to rest a change of name? We might adduce other examples, but we deem it unnecessary. We wish that Sir James had corrected this fundamental error. We fear that it will lead in Russia to what has already occurred in this country, that, should the College of Medicine of St Petersburgh publish a new edition of their Pharmacopœia without adopting Sir James Wylie's views on points of theory, there will be in use a mixed nomenclature of the most absurd and embarrassing description. If Sir James Wylie sees the London Medical Gazette, and will look back to some of the numbers for October and November 1840, he will observe the surgeon of a metropolitan infirmary, in recording a case of extirpated ovarian cyst, talk of giving *Chloride of Morphia*; and may see almost every week some person writing *Iodide of Potassa*; and then let him reflect on the uniformity, distinctness, precision, and scientific elegance, which, thanks to the London College of Physicians, the use of a scientific system of nomenclature has introduced into the writings of practical men.

Sir James Wylie's Pharmacopœia is divided into three portions:

I. MEDICAMENTA VEGETABILIA ET ANIMALIA.
II. MEDICAMENTA MINERALIA ET CHEMICA.
III. PRÆPARATA PHARMACEUTICA ET FORMULÆ.

The first being a description of vegetable and animal simples; the second containing mineral substances, and the more strictly chemical processes; the third being chiefly galenical formulæ.

In the first department we were struck chiefly with the variety of herbs taken notice of, which have long since become obsolete in this part of the world; such as *Bidens tripartita*, *Thymus serpyllum*, *Saponaria officinalis*, and, we may add, of animals *Formica rufa*. A great number of these, however, are extensively used among the peasantry, which is, we presume, the reason of their being noticed by Sir James.

We should say that, on the whole, the botanical part of the work has been done with very great care. There are, however, one or two errors which might have been corrected. For instance, as in the London Pharmacopœia, *Cinchona Lancifolia*, *Cordifolia*, and *Oblongifolia*, are said to furnish respectively the pale, yellow, and red barks, notwithstanding that the evidence to the contrary has long been acknowledged to be decisive. In like manner, *Convolvulus Jalapa*, Linn. *Ipomœa Macrorrhiza*, Mich., and *Ipomœa Jalapa*, Coxe, are given as synonyms of the jalap plant. Yet it is certain, from the description, that Michaux's plant, and probably that of Coxe, are neither of them identical with the true jalap plant, the *Ipomœa Purga* of Wenderoth.

Under each vegetable and animal medicine, Sir James has given a full account of the most important points respecting the qualities of the substance and its value as a drug. He has bestowed great labour in collecting a large body of facts and observations; the only fault which we have to find with this being, that he has been rather liberal in the ascription of virtues to some of his medicines.

The second department, the Mineral and Chemical Remedies, is the best part of the work. Especial care seems to have been taken with this portion, and most of the directions for the chemical operations are not only minute, but clear and explicit. We cannot afford space for going into a minute consideration of the various processes; but we shall mention a few which admit of a brief comment.

Under the head of distilled vinegar, we have a very neat process for procuring vinegar by the oxidation of weak spirits; a process which supersedes the necessity for distilling the vinegar, and has of late years been introduced into Germany by MM. Wageman and Schuzenbach. We translate the whole passage, as it is very interesting, and has not, so far as we know, been made extensively known in any work, except the newly published volume of Chimie Organique by Professor Liebig. We prefer Sir James Wylie's account of the process, as being the more distinct and precise of the two.

" In place of what is distilled, we may obtain vinegar prepared from spirit oxydated by the influx of air. The following is the construction of the apparatus necessary for this process :—Take an oak cask 5 or 6 feet high, place it perpendicularly in an apartment of about the temperature of 65° Fahr., and pierce in the circumference eight holes about an inch in diameter, at a distance of 15 inches from the bottom. A wooden disk is to be fitted into the interior of the cask, 5 inches from its upper edge, and fitted water-tight to the inside of the cask by means of tow. In the disk itself four large holes are to be pierced, half an inch in diameter, and more than 300 smaller holes, about half a line in extent. In each of the larger holes is fitted hermetically a glass tube, (open at both extremities,) and 3 inches long, through which egress is afforded to the air entering by the eight apertures below. Through the smaller holes are led thick flaxen threads, descending to the bottom of the cask, and retained above by a knot, through which liquid may flow down in an insensible stream. The space through which the threads descend between the disk and the bottom of the cask is almost totally filled with the smaller stalks of grapes, or fine shavings of wood, such as made by a plane, or washed birch twigs. In the lower part of the cask, 10 or 12 inches from the bottom, is made an aperture with a tube, to admit of the efflux of the vinegar. When thus arranged, the apparatus is first moistened with strong vinegar, of good quality, and then weak corn spirit, diluted with four parts of water, is transmitted down through the apparatus in a fine stream or in drops. One part of expressed carrot juice is sometimes added, by which the oxydation is much accelerated. If the vinegar is not sufficiently acidified by one transmission, the process may be repeated a second, or, if necessary, a third time."

We do not see the use of the threads going to the bottom of the cask, *(usque ad fundum descendentia)*, if the twigs or shavings are to be of any use in exposing the fluid freely to the air. It does not appear, from Liebig's description, that they require to go further than through the whole thickness of the disk. It is of essential moment in this process that there be a free supply of air to the apartment. The use of the carrot juice and old vinegar is this, that alcohol itself is not susceptible of acidification from the contact of oxygen, unless organic matters are present; but if these exist in the fluid, they act the part of intermediums in communicating oxygen to the spirit, in the same way, Liebig remarks, as the binoxide of azote aids the oxygenation of sulphurous acid in the leaden chamber of the vitriol-maker.

Under the head of *Aqua*, Sir James gives us a full account of the potatorial and other uses of water; and under *Aquæ Minerales* a very neatly arranged series of tables are given, showing the composition of a great many of the best known mineral springs, with a good many in Russia whose reputation is not so extended.

The process for sulphate of quinine is not a good one. The bark is ordered to be boiled with muriatic acid in water, the latter being in the proportion of 45 lbs of water to 4 oz. of bark. The mixed decoctions are to be precipitated when warm, with 4½ oz. of caustic lime suspended in 2 lbs of water. As no mention is made of concentrating the fluids, a large amount of quinine must be lost, for this alkali is much more soluble in water, especially when warm, than Sir James seems to be aware of. Moreover, the latter stages of the process, where the alcohol might be dispensed with, are much more complicated than is at all necessary for the production of pure sulphate. For these reasons this process can never be well adapted for the manufacturer.

Neither is the selection of Dr A. Todd Thomson's process for muriate of morphia judicious. The muriate of baryta, which is employed, has no advantage over the far cheaper muriate of lime. The making a fluid extract of opium, and diluting this with water, is quite unnecessary. A simple infusion, made at once by heat, answers quite well, especially if the first mass of crystals is well squeezed in the screw-press.

The *Aqua Potassæ*, the *Potassa liquida* of this Pharmacopœia, is a very different preparation from that of our British Pharmacopœias. It is evaporated till it has acquired a density of 1330. The specific gravity of that of the Edinburgh Pharmacopœia is only 1072. The latter, the strongest of the British preparations, contains, according to Dalton, only about 5 per cent. by weight of potassa, whilst Sir James Wylie's solution should contain no less than 26.3. We do not see the necessity or advantage of this concentration. It appears from what follows to be intended as a liquid caustic; but it can never answer this purpose so well as strong sulphuric or nitric acid.

With the exception of these and one or two other objectionable processes, we beg to express our gratification with this portion of the *Pharmacopœia Castrensis.* We have perused it with considerable attention, much interest, and not a little profit. We refrain from expressing an opinion on some of the processes which are new to us, and of which we have had no experience; but in the meantime, we have derived from the perusal some useful hints, for which our best thanks are due to Sir James Wylie.

The third part of the work, Pharmaceutical Preparations and Formulæ, has contributed to add unnecessarily to the bulk of the book. We hold that the introduction of a great many formulæ, for prescriptions which the physician ought to be ready at all times to construct for himself extemporaneously, is not judicious. We dislike this system of prescription made easy,—this *vade mecum* kind of practice. It tends to engender a routine style of work,—a slovenly way of prescribing,—a want of reflection and consideration as to the best forms of administration. We have seen the bad effects of such formulæ frequently manifested in the case of the prescribing pupils of dispensaries and similar institutions, where we have often seen complicated mixtures ordered, two-thirds of the ingredients of which were unnecessary for the treatment of the case, and for the selection of which no better reason could be assigned than that the formula was to be found in *Paris's Pharmacologia*, the formulary of *Copland's Dictionary*, or *Pereira's Selecta e Praescriptis.* The formulæ which ought to be in a Pharmacopœia are those which require to be made or kept by the apothecary; either from their being in constant use, as some pills and powders, or from their requiring some time for their preparation, as tinctures, wines, vinegar, extracts, &c., or from their requiring certain proportions to be observed in their preparation, as many infusions and decoctions, and instances of double decomposition, as the *Solutio Acetatis Zinci.*

Of these we have a great many in the present work, which are excellent and well-selected. The following amongst many others deserve notice:

Baccæ Copaiferæ Factitiæ, gelatinous copaiva capsules, so extensively used on the continent in administering this not very pleasant balsam. ***Decoctum Cinchonæ Acidulum*** ; by the addition of the dilute sulphuric acid, the whole active principles are more completely dissolved and removed, and the deposit which takes place on the cooling of the ordinary decoction is prevented. ***Extractum Filicis Maris Ethereum***, the oil of Filix-Mas, an anthelmintic deserving a more extended use than it has met with in this country. ***Linteum Adhesivum*** (a bad name by the way), meaning the isinglass plaster so extensively used by Mr Liston for dressing wounds.

Among the formulæ which will appear somewhat novel to English readers are the various combinations of herbs chiefly for making infusions and decoctions, bearing, as they always do, in the Foreign Pharmacopœias, the title of *Species*, with an accompanying adjective indicative of the qualities ascribed to them. Thus we have *Species Pectorales*, containing marsh-mallow, coltsfoot, sage, elecampane, liquorice, poppy-heads, and fennel ; *Species Antiscorbuticae*, containing fir-tops, milfoil, wormwood, marsh trefoil, juniper berries, and *Calamus aromaticus*. It appears to us rather curious to see these *herb teas* in a scientific work. In this country, the compounding and administration of such formulæ has in a great measure fallen into the hands of old women.

Want of space compels us to refrain from quoting any of the formulæ for prescriptions, many of them no doubt excellent. We retain our opinion as to the general inexpediency of such formulæ being published at all.

We conclude our notice of Sir James Wylie's book, by again expressing our admiration of it as a whole. We have in its perusal had much pleasure as well as instruction, and shall be happy to avail ourselves of many of the hints which it contains. We have derived additional gratification from this book, on account of its being the work of a countryman abroad, who has contributed to elevate the character and maintain the dignity of British medicine in a foreign land, and who, though by the exertions and good fortune of his earlier years he has attained an unusual degree of exaltation in his adopted country, has chosen the honourable part of continuing to labour in an extensive and important field of professional study, instead of folding his arms, and sitting down to enjoy his *otium cum dignitate*.

ART. II.—*Odontography, or a Treatise on the Comparative Anatomy of the Teeth ; their physiological relations, mode of developement, and microscopic structure, in the Vertebrate Animals, illustrated by upwards of* 150 *Plates.* By RICHARD OWEN, F. R. S. Hunterian Professor to the Royal

College of Surgeons, London, &c. &c. Part 1. London, 1840. Pp. 112, and 50 Plates.

Mr Owen has been long favourably known to the scientific world for his varied researches in comparative anatomy; and the present work, on the Teeth of the Vertebrated Animals, so far as a judgment may be formed from the small portion before us, bids fair to sustain the reputation which he has already acquired both in this country and on the continent. It appears to be an extension of his paper "on the Structure of the Teeth," read before the British Association at Newcastle in August 1838, an abstract of which was published in the seventh volume of their Reports. In that paper he endeavoured to show that the general tendency of the modifications observable in descending from man to the lower classes of the vertebrated animals, was a nearer approximation of the substance of the teeth to the vascular and organized texture of bone; and his chief and strongest reason for arriving at this conclusion appeared to be, because the microscope showed that the teeth of all these animals were composed of tubular structures,—structures which also constituted no inconsiderable portion of the solid bones. The fallacy of this conclusion will be adverted to afterwards.

The present portion of his work is devoted to the dental system of fishes, and commences with a few general observations on the varieties in their number, form, situation, attachment, substance, chemical composition, structure, and developement.

Mr Owen remarks, that "the greater number of fishes have their teeth composed of an osseous substance, somewhat denser than the jaws to which they are affixed. In some instances, as in the teeth of the flying-fish (*Exocœtus*), and sucking-fish (*Remora*), the substance of the tooth is uniform, and not covered with a layer of denser texture. In others, as the shark, sphyræna, &c. the tooth is coated with a dense shining enamel-like substance; but this is not true enamel, nor the product of a distinct organ; it differs from the body of the tooth only in the greater proportion of the earthy particles, their more minute diffusion through the gelatinous basis, and the more parallel arrangement of the calcigerous tubes; but it is developed in and by the same matrix, and, resulting from the calcification of its external layer, is the first part of the tooth which is formed. In the *Sargus* and *Balistes*, the dentine, or proper osseous substance of the tooth, is harder than that of the fishes last cited, and is covered with a thick layer of a denser subetance, developed by a distinct organ, and differing from the enamel of the higher animals only in the more complicated and organized mode of deposition of the earthy particles. The ossification of the capsule of the matrix gives the enamel of the teeth of the file-fish, and some others, a thin coating of a third substance analogous to the "cæmentum, or crusta-petrosa" of the mammalian teeth. And in the pharyngeal teeth of the parrot-fish a fourth substance is added to the structure of the tooth by the coarser ossification of the pulp, after its peripheral part has been converted into the dense ivory. The teeth, consisting of dentine, enamel, cement, and coarse bone, are the most complicated as regards their substance that have yet been discovered."—P. 8, 9.

In speaking of the structure of the teeth, it is remarked that the tubular structure is common to the teeth of fishes as well as to those of all the other classes of vertebrated animals; and that four principal modifications of this structure occur.

Premising that the essential character of this structure is the presence of a *cavitas pulpæ*, or medullary canal, from which the calcigerous tubes radiate, the *first* modification, he remarks, is observed in the rostral teeth of the saw-fish, where the tooth is traversed by a number of equidistant and parallel medullary canals, each canal, and its system of medullary tubes, representing a cylindrical or prismatic denticle, which is separated from the contiguous denticles by a thin coat of bone or cement. Occasionally, as in the teeth of the parrot-fishes and chimæra, the contiguous medullary canals anastomose together.

In the *second* modification the substance of the tooth is traversed by medullary canals, somewhat less regularly equidistant and less parallel than in the first, having the boundaries of their respective systems of radiated calcigerous tubes indicated by the minute calcigerous cells with which the terminal branches of those tubes communicate. These boundaries are more or less obscured by the terminal branches of the calcigerous tubes crossing into the interspaces of the corresponding branches of an adjoining system of tubes, and anastomosing with them immediately or through intervening dilatations or cells. The medullary canals often dichotomize, and anastomose more frequently than in the first modification. The teeth are generally of a large size; and a good example occurs in the Port-Jackson shark, *(Cestracion Philippi.)*

The *third* modification is the most common and characteristic of the dental structures of fishes. The tooth is permeated by a network of medullary canals, of which the interspaces are occupied by the calcigerous tubes or cells. The medullary tubes are directly continuous with those of the common bone with which the tooth is anchylosed. As these proceed through the tooth they maintain a course more or less parallel, and more or less straight or wavy; but they ramify abundantly, and gradually diminish in calibre as they approach the surface of the tooth. This form of structure is seen in the perch, salmon, herring, and other families of fishes.

The *fourth* modification is that which approaches nearest to what is met with in the higher classes of vertebrated animals, and is characteristic of the teeth of most of the reptiles and of the Mammalia. The tooth consists of a single medullary or pulp canal, and a single system of calcigerous tubes, radiating from the central canal at right angles to the periphery of the tooth. This structure is met with in the teeth of the extinct sauroid fishes, in the file-fish *(Balistes)*, angler *(Lophius)*, &c.

Mr Owen takes occasion to state his opinion as to the vitality of the teeth in the following words:

> " The uniform result of my researches on the structure of the teeth in all grades of vertebrate animals, and in their natural and diseased states, has been a conviction of the untruthfulness of the terms inert, dead, and unorganized, as applied to the substance of any tooth whatever. Extra-vascular undoubtedly is all that portion which consists of the calcigerous tubes; the capillary circulation is confined to the pulp or medullary canals; but since every secretive process and the developement of the primordial cells of every tissue are due to changes produced in the *liquor sanguinis* transuded from and beyond the sphere of the ultimate capillaries, the absence of these vessels in the dense dental substance is as little conclusive against its vital and organized nature, as it would be to prove the inert condition of the germinal membrane of the ovum before the thirtieth hour of incubation."—P. 13.

In the fifty-third volume of this Journal the question as to the vitality of the teeth was fully discussed, and the conclusion arrived at was, that the teeth, being destitute of blood-vessels, nerves, and absorbents, and no changes occurring in them which could fairly be attributed to a vital agency, they ought to be regarded as destitute of vitality.

After an impartial examination of their structure, growth, &c. it was also stated, that teeth ought to be classed along with the other cuticular appendages, as hair, feathers, claws, spines, &c.; organs which, though organized, as being the result of an organic secretion, were at that time usually regarded as destitute of vitality. That the opinion then expressed, regarding the class of organs to which teeth ought to belong, was correct, the researches of Professor Owen fully establish. With regard to what conclusion he himself may draw from the facts which he states in the present work, we have no means of ascertaining, the work not yet being completed; but as from the same facts, in his paper read before the British Association, he concluded that teeth ought to be arranged in the same category as bones, and this from both these organs possessing a somewhat similar tubular structure, it may be necessary to advert very shortly to the arguments in favour of the opinion expressed in the former volume of this Journal.

All organs secreted by the cuticular or dermoid tissue are composed of a tubular structure, either in single tubes, as seen in hair, or in various forms of aggregation and modification, as observed in nails, feathers, claws, bills of birds, spines of fishes, &c.; nay, the very epidermis itself is composed of tubular scales; in fact, the essential structure of the cuticular surface, and of all its appendages, is the tubular structure. The microscope shows that many of the spines of fishes possess in all respects the identical minute structures observed in the teeth of the same animals; and the very formation of these two classes of organs is now proved, beyond the possibility of a doubt, to be precisely similar. Hair, spines, feathers, &c. all grow from vascular papillæ or bulbs on the surface of the dermoid tissue. The researches of Muller, Arnold, and Goodsir, have proved that the human teeth are formed from papillæ of the mucous surface of the mouth; and every fact regarding the dentition of the lower animals, and especially the varied and extensive researches of Professor Owen, clearly demonstrate that these organs arealways produced from papillæ formed on the surface of the mucous or dermal tissues. The teeth of the saw-fish, in fact, are spines, formed on the dermal surface; and yet in no respect, either in their mode of growth, or internal structure, do they differ from true teeth. So long as the facts were limited to the examination of the human teeth, doubts may have remained as to whether they ought to be regarded as bones, or appendages of the skin; but the examination of these organs in the lower classes has now brought so many new facts to light, that not a doubt can remain as to which class they ought to belong. So that whether these organs be examined in their intimate internal structure, in their mode of developement and growth, or in their relations to the other organs of the body, they must be regarded as simple cutaneous appendages.

As the subject of the developement of the teeth is one of the most important parts of the general subjects treated of in the present work, it is imposssible to pass it over without quoting the details.

"In all fishes, as in other vertebrate animals, the first step is the production of a simple papilla from the free surface of either the soft internal integument, as in the young *Pristis*, or on the mucous membrane of the mouth, as in the rest of the class. In these primitive papillæ there can be early distinguished a cavity containing fluid, and a dense membrane, *membrana propria pulpi*, surrounding the cavity, and itself covered by the thin external buccal mucous membrane, which gradually becomes more and more attenuated as the papilla increases in size. In some fishes, as the sharks and rays, the dental papillæ do not sink into the substance of the vascular membrane from which they grow, but become buried in depressions of an opposite fold of the same membrane: these depressions enlarging with the growth of the papillæ, and forming the cavities or capsules in which the developement of the tooth is completed. They differ from the capsules of the matrix of the mammiferous tooth in having no organic connection with the pulp, and no attachment to its base; the teeth when fully formed are gradually withdrawing from the above described extraneous capsules, to take their place and assume the erect position on the alveolar border of the jaws.

" Here, therefore, is represented on a large and, as it were, persistent scale, the first and transitory papillary stage of the developement of the mammalian teeth; and the simple crescentic cartilaginous maxillary plate with the mucous groove behind it containing the germinal papillæ of the teeth, offers in the shark a magnified representation of the earliest condition of the jaws and teeth in the human embyro.

" In many fishes, as the lophius and pike, the dental papillæ become buried in the membrane from which they arise, and the surface to which their basis is attached becomes the bottom of a closed sac. But this sac is never lodged in the substance of the jaw, the developement of the tooth being completed in the tissue of the thick and soft gum or mucous membrane from which the papillæ were originally developed: hence teeth in various stages of growth are frequently brought away with that membrane when it is reflected from the jaw-bone. The ultimate fixation of the teeth, so formed, is effected by the developement of ligamentous fibres in the submucous tissue between the jaw and the base of the tooth; which fibres become the medium of connection between those parts, either as elastic ligaments, or by continuous ossification.

" Here we have the second step in the developement of the mammalian tooth represented, viz. the imbedding of the pulp in a follicle of the mucous membrane; but the eruptive stage of the tooth takes place without any previous inclosure of the follicle and pulp in the substance of the jaw.

" In the *Balistes, Sparoids, Sphyræna, Scarus,* and many other fishes, the formation of the teeth presents all the usual stages which have been observed to succeed each other in the dentition of the highest organized animals; the papilla sinks into a follicle, becomes surrounded with a capsule, and is then included in a closed alveolus of the growing jaw, where the developement of the growing tooth takes place, and is followed by the usual eruptive stages.

" The developement of the dental pulp in fishes, prior to the deposition of the calcareous particles in it, corresponds in the main with the processes described by Purkinje and Raschkow in the mammalia. The pulp-substance, or contents of the *membrana propria* remain, in fishes, for a longer period in a fluid or semifluid state, and the granules or nucleated cells which are first developed, float loosely or in small aggregated groups in the sanguineo-serous fluid: they first attach themselves to the inner surface of the *membrana propria,* if these be not originally developed from that surface, and the whole of the contents of the growing pulp becomes soon after condensed by the numerous additional granules which are rapidly developed in it after it has become permeated by the capillary vessels and nerves. The arrangement of these particles into linear series, or fibres, is first observable at the superficies of the pulp to which the fibres are vertical; and, at this period, ossification has commenced in the dense and smooth *membrana propria* of the pulp; it is thence continued centripetally in the course of the above-mentioned lines, towards the base of the pulp, either regularly progressive, as in the incisors of the *Sargus* and *Balistes*, or radiating, as in *Sphyræna*, and (if we may judge by *à posteriori* observation of the structure of the fully developed teeth) in most other fishes, from the various centres formed by the persistent capillaries of the pulp, around which the cells or granules become condensed into concentric layers, which then become, as they are successively impregnated with the calcareous salts, the walls of the medullary canals.

"In the shark, and all those fishes in which the teeth are completely formed without going beyond the papillary stage of developement, there is no distinct enamel pulp; the dense exterior layer of the tooth is formed by the calcification of the *membrana propria* of the pulp, which, therefore, precedes the formation of the ordinary dentine. But in the file-fish *(Balistes)*, the sargus, the gilt-head *(Chrysophrys)*, and some other fishes, a conspicuous enamel-pulp is developed from the inner surface of the capsule which surrounds the bone-pulp; this enamel organ terminates, as in the human subject, before the capsule is reflected upon the base of the pulp. It has a firmer tissue, more closely resembling that of the ordinary pulp, than in the mammalia: and, when examined under the microscope, presents numerous and close-set fine fibres near that surface which is next the bone-pulp, and to which these fibres are generally placed at right angles. The base of the enamel organ, which is attached to the capsule, presents a granular and fibrous tissue blended together. I have not been able to trace any capillaries from the capsule into the substance of the enamel-pulp. In the incisors of the sargus, the developement of the enamel and dentine begins simultaneously upon the contiguous surfaces, and when we observe how close and compact is the package of the matrix of the tooth in the alveolar cavity of the jaw, it is hardly possible to conceive how either of these substances could be the product of transudation from their respective pulps. It is, however, easier to separate the primary layers of the enamel and dentine from their respective pulps than from each other; yet if the denuded surfaces of the uncalcified portions of the pulps be examined by reflected light under a compound lens of a half-inch focus, they are seen to be ragged and punctate, and evidently different from the original surfaces prior to the commencement of the deposition of the calcareous salts in them. The formation of the enamel resembles more closely that of the dentine in the fishes cited than it does in the mammalia, and the enamel contains a greater proportion of persistent animal matter.

"The course of calcification of the two pulps takes opposite directions, and in the *Balistes*, the process finishes by the ossification of the outer layer of the capsule itself, by which both the enamelled crown and the base of the tooth are coated with a thin layer of bone. I have not been able to discern any radiated cells in this analogue of the *crusta petrosa*, or cement of the mammalian teeth. It soon wears off from the crown of the extruded tooth.

"In all fishes, the teeth are shed and renewed, and this not once only, as in most mammalia, but frequently, and during the whole lifetime of the animal. Fishes, indeed, can hardly be said to have permanent teeth. The rostral teeth of the pristis constitute, perhaps, the sole exception; and these may be regarded rather as modified dorsal spines.

"In all cases where the first teeth are developed in alveolar cavities, the succeeding ones follow them in the vertical direction, and owe the origin of their matrix to the continuation, from the mucous capsule of their predecessors, of a cœcal process, in which the papillary rudiment of the dental pulp is developed. But in the great majority of fishes, the germs of the new teeth are developed, like those of the old, from the free mucous membrane of the mouth through the whole period of succession, a condition which is peculiar to the present class."—Pp. 14–19.

After these preliminary observations Mr Owen passes to the consideration of the structure of the teeth in the different orders, genera, and species of fishes; his varied remarks on each being illustrated by beautiful lithographic plates, exhibiting both their external form and magnified views of their internal minute structures.

The teeth of the *Plagiostomata*, or cartilaginous fishes, appear particularly to have attracted the author's attention. After some general remarks on the present state of our knowledge as to the bones which enter into the formation of the jaws of this class of fishes, and showing how other comparative anatomists have mistaken the different modifications in the structure of these parts, Mr Owen arrives at the conclusion, that the "dentigerous cartilaginous arches of the sharks and rays represent, the one, the combined maxillaries and intermaxillaries, the other, the confluent articular and dentary elements of the lower jaw."

The teeth of the cartilaginous fishes are not immediately connected with the cartilaginous arches, are never implanted in maxillary alveolar cavities, nor are they confluent with the substance of the jaw, even when the external crust of the jaw is ossified, but are always attached to the fibrous and mucous membranes which cover the maxillary cartilages. Retaining their common and characteristic type of structure, the teeth of these fishes exhibit every grade of modification of form, from the laniary to the molar type, suited to the varied habits of the animals, and the kinds of food on which they subsist.

In all the sharks the body of the tooth is principally occupied with two kinds of canals, which Mr Owen terms medullary and calcigerous, the latter being, essentially, minute branches of the former. In the newly-formed tooth, these are distinguishable by the nature of their contents; but the characteristic marks of each are gradually obliterated by the progressive developement of calcareous matter by concentric layers in the medullary canal.

The formation of the teeth of the sharks exemplifies on a large scale the earliest or papillary stage of dental developement seen in the higher classes of animals. It is not, however, succeeded either by the follicular or eruptive stage; the formative papillæ are never enclosed, and consequently never break forth. The pulp, when consolidated by the deposition of calcareous salts in the pre-existing cells and tubes, is gradually withdrawn from the protective sheath which the thecal fold of mucous membrane afforded it during the early stage of its developement. Mr Owen thus describes the particular appearance of these structures in the uterine fœtus of the common white shark:

"A fissure presents itself on the inner side of the margin of each jaw, running parallel with it, between the thin smooth membrane covering the convex edge of the cartilage, and the free margin of a fold of mucous membrane which lies parallel to, and upon the inner edge of the jaw. When this fold is drawn away from the jaw, the minute teeth are exposed, arranged in the usual vertical rows; their points are all directed backwards and towards the base of the jaw, and are seen to slip out of fossæ, or sheaths in the membranous fold, as this is gradually reflected backward to its line of attachment near the base of the jaw. Here the anterior lamina of the fold, which, from its office, may be termed thecal, is continuous with the mucous membrane at the base of the rows of teeth; the posterior layer is reflected backwards to the frœnal line of attachment of the tongue. Close to the anterior line of reflection there is a row of simple conical papillæ; in the succeeding row, the papillæ are larger, the cone broader and flatter, and its apex is covered with a small cap of dense and glistening dental substance, which is readily removed; though not without displacement of part of the pulp, the granules of which, adherent to the cavity of the displaced dental cap, are always readily recognizable under the microscope. The third series of papillæ, counting from below in the lower jaw, have acquired the size and shape of the future tooth, with the crenate edges well marked; half the tooth is completed, and its removal from the fleshy base of the pulp cannot be effected without evident laceration of the pulp; when this is done under the microscope, the torn processes of the pulp continued into the medullary canals of the new formed tooth are plainly visible. The fourth tooth is completely formed, as also the fifth and sixth, in the ascending series; these progressively diminish in size. The last or highest, which is first exposed on reflecting the thecal fold, and the first which is completed in the order of developement, consists of a simple cone, similar in form and size to the apical third of the ordinary sized teeth below it; yet its growth is quite completed, and its base firmly attached to the maxillary membrane."

" The unossified pulps, examined with a higher power, consist of semi-opaque polyhedral granules or cells suspended in a clear *matrix*, and the whole enclosed in a tough transparent membrane, which forms the outer surface of the pulp. Beneath this membrane, at the crenate margins, the granules or cells are arranged in lines precisely corresponding with those of the subsequent calcigerous tubes. The formation of the tooth commences by the deposition of earthy particles in the tough external membrane of the pulp. I have been unable to recognize the distinct arrangement of the hardening salts in this layer. It is transparent, extremely dense, and forms the enamel-like polished coating of the tooth; in sections of fully formed teeth, the finest terminal branches of the parallel peripheral calcigerous tubes are lost in the above clear enamel-like substance. When the enamel-like outer layer of the apex of the tooth is completed, it is so easily detached from the subjacent pulp that it might be readily supposed that there was no organic connection between them. If, however, the so exposed pulp be now examined with the microscope, and compared with an uncalcified pulp, it is seen to be no longer covered with the smooth dense membrane observable in the latter; but the apical edges, from which the enamel-like cap has been detached, appear villous or floccular. It is obvious, that the first shell of the tooth has been neither transuded from the superficies of the external membrane of the pulp, nor has been deposited between that membrane and the granular part of the pulp, but is due to a conversion of the external membrane into a dense enamel-like bone. The formation of the body of the tooth by deposition of earthy particles in pre-existing and pre-arranged cavities is still more satisfactorily demonstrable. In proportion as the formation of the tooth has advanced, the difficulty of separating the calcified from the uncalcified portion of the pulp is increased, and at the same time it becomes easier to detect the continuation of the processes of the pulp into those medullary canals which form so many centres of radiation of the plexiform calcigerous tubes."—Pp. 35–37.

" As a consequence of a formation of a tooth by *conversion of*, instead of *transudation from*, a pre-existing pulp, the successive formation of these pulps necessarily follows, where a succession of teeth is required; these reproductive pulps are developed in the shark in the vascular mucous membrane at the angle of reflection of the thecal fold upon the groove at the basal line of the jaws. They gradually advance from this situation towards the margin of the jaw, the centripetal ossification extends as they advance, and consolidation is completed by the time they are ready to change their recumbent for the erect position, and take the place of the tooth previously shed."—P. 39.

Mr Owen has rendered it extremely probable that the change of place, and direction of the teeth of the sharks, depends on some process of partial absorption and deposition operating on the membrane to which the teeth are attached, but not on the jaw; consisting in a slow and gradual sliding motion of the dentigerous membrane upon the jaw. The fact upon which this opinion is mainly founded is, that in the jaw of a *Galeus*, which had been penetrated with the barbed spine of a sting-ray, which had broken off and remained fixed in the jaw, a double row of imperfectly formed teeth was found continued from the internal surface of the perforated part of the jaw to the margin supporting the erect teeth; a circumstance which Mr Owen considers to prove that the dentigerous membrane advanced, whilst the particles of the jaw remained stationary.

The teeth of the *Myliobates* or rays, are, like those of the shark, formed at the posterior part of the tesselated series, in proportion as they are worn away in front. A series of minute and closely aggregated papilliform matrices rise from the mucous membrane behind the teeth, and are covered by a fold of the same membrane, which is reflected forwards, so as to conceal the pulps and last formed teeth. The papilliform pulps are ossified by the deposition of the calcareous salts in the peripheral cells and radiating tubes, but the medullary or central canal of such pulp continues to retain its vascular contents, till the whole of the compound tooth is completed. The calcified wall of the medullary canal is then thickened, and the area diminished by the successive formation of concentric laminæ of osseous matter.

The peculiar formation of the jaws of the *Cestracion*, or Port-Jackson shark, is next noticed, approaching, as they do, in their greater elongation and more horizontal position, to those of the osseous fishes. The author shows that the upper dentigerous arch represents the ordinary maxillary and intermaxillary bones, whilst the labial cartilages or their rudiments, thought by Cuvier to represent in the cartilaginous fishes the maxillary, intermaxillary, and premandibular bones, are entirely awanting.

In the teeth of this fish, when the dense outer layer is removed from the crown of the newly formed teeth, the orifices of the medullary canals perforating the whole body of the tooth, are brought into view. These tubes or canals are more or less occupied in the recent fish by a vascular pulp, and are continued directly from the irregular cells and canals of the semi-ossified crust of the jaw. In the large crushing teeth, the greater number of the medullary canals proceed in pretty regular and slightly wavy courses towards the grinding surface, whilst the outer ones incline towards the lateral surfaces ; but they soon begin to divide, and the divisions continue to ramify dichotomously. The branches anastomose, particularly near the surface, and each maintains nearly the same size as the trunk. The process of dentition is most clearly one of conversion not of excretion ; the earthy particles being deposited in the microscopic cells and tubes of the formative matrix.

The teeth of the various families included under the title of *Ganoid* fishes next engages Mr Owen's attention. In most of these there is but little to interest the general reader ; though the minuteness and precision of his descriptions, together with the beautifully drawn lithographic plates, cannot fail to prove of the highest utility to the student of natural history, and especially to the geologist, in the elucidation of the extinct species.

In speaking of the compound teeth of the *Diodon*, Mr Owen takes occasion to correct the mistake into which Cuvier and Von Born had fallen regarding their structure and mode of formation. The teeth of the diodon were supposed by these naturalists not to be formed by the deposition of calcareous tubes in the pulp's substance, but by the apposition or transudation of layers of calcareous matter from the pulp's surface. The exposed surface of the tooth of the diodon presents, in fact, a series of transverse and parallel striæ, which, in a vertical section, are seen to be the margins of thin, superimposed, horizontal, and slightly flexuous plates, which have been partially abraded by trituration in an oblique plane. The superior layers are the most worn, and are evidently the oldest ; in proportion as they descend, in the lower jaw, they increase in breadth, and, finally, instead of being soldered together, they become detached, thinner, and of a more friable texture ; the lowest and incompletely developed plates lie loosely in the cavity of the jaw beneath the superincumbent dental mass. It was this peculiar appearance which deceived Cuvier and Von Born. The mode of formation of the tooth, however, is thus described by Mr Owen :

" The mucous membrane of the mouth and periosteum of the jaws are reflected into the cavities at the base of the compound tooth ; the periosteum lines the parietes of the cavity, and the mucous membrane forms a thick cushion extending across its floor. From this surface a lamelliform pulp is developed, in which the calcifying process takes place in a direction from above downwards. At first the earthy salts are deposited in the state of such minute subdivision, and in such a direction and abundance, as to produce the dense and minutely tubular structure of the dental plate. When this has acquired its due thickness, the rest of the pulp becomes ossified, *i. e.* the calcareous salts are deposited in less abundance, and in the parietes and interspaces of coarse cells, instead of those of minute tubes. The margins of the ossified pulps, by this process, become confluent with the parietes of the general dental cavity, and the mutual adhesion of the flattened surfaces of the impacted lamelliform teeth is promoted by the pressure to which their exposed surfaces is subject. By the time that ossification has begun in one pulp, a second has been developed beneath it, and it is the portion of the pulp solidified by the fine tubular calcification which gives rise in the macerated and dried jaws to the bone and thin lamellæ in the dental cavity. These lamellæ become fixed by means of the coarser calcification or ossification which subsequently takes place in the remains of the pulp, and their margins are thus anchylosed to the surrouuding bone, in a manner analogous to the fixation of the base of the ordinary shaped teeth in other fishes."—P. 79.

In the compound tooth of the diodon, then, parallel and aggregated series of short calcigerous tubes are separated by thin layers of a cellular bone. But the lamellæ in it, as well as in the tusk of the elephant, and the conical molar tooth of the cachalot, present an organized structure of aggregated calcigerous tubes, directed more or less at right angles to the plane of the lamellæ, and indicate that higher mode of developement by calcification of the pulp, which it is the chief object of Mr Owen's researches to exemplify.

In the published portion of this work the dental characters of another group of fishes is given, viz. the *Ctenoid* fishes, consisting chiefly of the Percoides, Gobioides, Squamipennes, and Pleuronectes of Cuvier; as also a portion of the first family of the Cycloid fishes, the Labroides of Cuvier.

Many and interesting are the details given on each of these groups, for which we must refer to the work itself, What we have said may serve to give some general idea of the interesting and original information here communicated, and what may be further expected from this accomplished anatomist.

ART. III.—1. *First Annual Report of the Registrar-General of Births, Deaths, and Marriages in England. Presented to both Houses of Parliament by Command of Her Majesty.* London, 1839.

2. *Second Annual Report of the Registrar-General of Births, Deaths, and Marriages in England, with Appendices. Presented to both Houses of Parliament by Command of Her Majesty.* London, 1840.

THESE reports, with the materials from which they are compiled, and the appendices with which they are accompanied, form valuable contributions to the statistics of the population of

England and Wales, and, if continued upon the same general principle with some improvements which their perusal has suggested, will doubtless elucidate many of the obscure and difficult questions relative to the influence of different modes of life upon health, and more especially upon those diseases which contribute most to form the great stream of human mortality.

It is unnecessary here to enter into a minute examination of all the facts and classes of facts which have been here brought forward, or of the general conclusions to which they may lead. But it may be proper to consider shortly some of those circumstances in vital statistics which are here established, and to discover what light they throw upon the salubrity and longevity of different kinds of human society, and upon the circumstances by which that salubrity and longevity are influenced.

The First Report embraces the year terminating on the 30th June 1838, and the Second Report embraces that terminating on the 30th June 1839. Both of these reports, by Mr Lister, are of a general nature, and present a general view of the whole results obtained from the registration of births, deaths, and marriages, for these two periods of twelve months each.

The appendices contain letters from Mr Farr reporting on the recorded causes of death for the latter half of the year 1837; and to this chiefly at present our attention shall be directed.

When an observer considers the immense number of human beings going out of the world every minute of the hour, every hour of the day, every day of the week, and every week of the year, he naturally interrogates himself whether the mode of death is the same in all, and whether every individual, who comes to this last termination, travels along the same road? He finds, upon a little inquiry, that the illness which had led to death is different in different individuals; that all forms of disease do not necessarily terminate in death; and that of those which have this termination, much greater numbers are destroyed by one kind of disease than by others. He is naturally led to distinguish diseases which are liable to terminate fatally, and even accidents and injuries which may have this termination, into various classes and orders. The ordinary nosological classifications of physicians may answer the purpose pretty well; but as none of these classifications are very perfect, it has become of late years a matter of consideration, whether, for statistical purposes, a peculiar method of registration and classification be not requisite. Almost all the arrangements attempted by nosological authors are in various modes imperfect and erroneous; and even supposing that they were more perfect than they really are, they are not well suited for statistical purposes. Even the nosology of Cullen, the most faultless, perhaps, of arrangements, is by no means well suited for the purpose now specified.

Mr Farr, who seems to have bestowed upon this subject much attention and study, has taken a different view of it, and has attempted to frame a statistical nosology upon principles, if not altogether new, at least better adapted to the purpose of ascertaining the nature of those multiplied disorders by which the health of mankind is impaired, and the duration of human life is abridged.

After distinguishing all the diseases incident to the human body into epidemic, endemic, and contagious diseases on the one hand, and sporadic diseases on the other, he enumerates under the former head all those maladies which are known usually to affect great numbers of persons at the same time, to prevail much at the same season, or to be very abundant in particular regions and districts. The latter class, viz. sporadic diseases, he distinguishes chiefly according to the different groups of organs which they affect. This arrangement will be best understood by the following table.

Statistical Nosology.—Fatal Diseases.

		Synonymes, Provincial terms, &c.	
Epidemic, Endemic, and Contagious Diseases.	SMALL-POX .	Variola.	Swine pox, water pox, hives.
	Chicken-pox . .	Varicella.	(1)
	MEASLES . . .	Rubeola.	
	SCARLATINA . .	Rosalia. Scarlet fever.	
	Putrid sore throat	Cynanche maligna. Ulcerated sore throat.	
	CROUP	Cynanche Trachealis.	
	HOOPING-COUGH	Pertussis. Chin-cough.	
	THRUSH . . .	Aphthae. Flox, frox, frost, white mouth.	
	DIARRHŒA . .	Looseness, purging, bowel complaint.	
	DYSENTERY . .	Dysenteria. Flux, bloody flux.	
	CHOLERA . . .	Cholera biliosa. Cholera infantum.	
	Asiatic Cholera .	Cholera epidemica.	
	INFLUENZA . .	Catarrhus epidemicus.	
	ERYSIPELAS . .	St Anthony's fire.	
	Puerperal fever .	Under childbed.	
	Mumps . . .	Cynanche parotidea.	
	TYPHUS* . . .	Febris typhoides.	Low fever, jail fever.
	Synochus . .	Inflammatory fever.	
	Miliaria . .	Miliary fever.	
	Gastric fever .	Febris gastrica.	
	Nervous fever .	Febris nervosa.	Brain fever.
	Bilious fever .	Febris biliosa.	
	Remittent fever .	Febris remittens.	
	Yellow fever .	Typhus icterodes.	
	Plague . .	Pestis.	
	SYPHILIS .	Lues venerea.	
	HYDROPHOBIA.		

---

* Mumps, miliary fever, remittent fever, yellow fever, and plague, are so rarely fatal in England that they have only been distinguished in the notes; and are referred in the Abstract, with all the modified species of fever, to typhus, as the number of fatal cases thus confounded with typhus is too inconsiderable to affect the resulting mortality. Scorbutus, purpura, scrofula, purulent ophthalmia, rheumatic fever, pellagra, cretinism, elephantiasis, Barbadoes leg, beribery, yaws, leprosy, malignant pustule, ergotism, hospital gangrene, when they occur in England, appear only to occur sporadically.

			Synonymes, Provincial terms, &c.
Sporadic Diseases.	*Of the Nervous System, and Senses.* Brain, Spinal Marrow, Nerves, Eyes, Ears.	CEPHALITIS	Inflammation of the brain. Phrenitis.
		Meningitis	Inflammation of membranes of brain.
		Tubercular meningitis	Idem with tubercle.
		Myelitis	Inflammation of the spinal marrow.
		HYDROCEPHALUS	Dropsy on the brain.
		CONVULSIONS	Convulsio, spasmi. Fit. (2)
		APOPLEXY	Apoplexia. Apoplectic fit.
		PARALYSIS	Palsy. Paralytic fit. Hemiplegia, paraplegia.
		TETANUS	Locked jaw. Trismus.
		CHOREA	St Vitus's dance.
		EPILEPSY	Falling sickness. Epilepsia. Fit.
		Hysteria	Hysteric fits. Cramp, spasms.
		Catalepsy	
		INSANITY	Mania. Madness.
		Monomania	Melancholy.
		Dementia	Demence.
		Idiocy	Idiotismus.
		DELIR. TREMENS	Delirium with trembling.
		Ophthalmia	Inflammation of the eye.
		Purulent ophthalmia	Ophthalmia purulenta.
		Iritis	Inflammation of the iris.
		Otitis	Inflammation of the ear.
		Neuralgia	Tic douloureux.
		DISEASE (softening, abscess, tumor, atrophy, &c.) of the brain, spinal marrow, nerves, eyes, or ears.	Morbus cerebri, cerebelli, medullæ spinalis, nervorum, oculorum vel aurium.
	Of the Organs of Respiration. Larynx, Windpipe, Air tubes, Lungs, Pleura.	QUINSEY	Cynanche tonsillaris.
		LARYNGITIS	Inflammation of the larynx.
		BRONCHITIS	Inflammat. of air tubes. Catarrh. Coryza.
		PNEUMONIA	Inflammation of the lungs. (3)
		PLEURISY	Pleuritis. Inflammation of the pleura.
		HYDROTHORAX	Dropsy of the pleura.
		Pneumothorax	Air between the pleura.
		ASTHMA	Emphysema.
		Hæmoptysis	Spitting of blood. (4)
		Phthisis	CONSUMPTION. DECLINE. (5)
		DISEASE (ulcer, abscess, &c.) of the larynx, air tubes, lungs, or pleura.	Morbus laryngis, bronchi, pulmonum.
		Nasal polypus	Polypus narium.
	Of the Organs of Circulation. Heart, Arteries, Veins, Lymphatics.	PERICARDITIS	Inflammation of the pericardium.
		Endocarditis	Inflammation of the internal membrane of the heart.
		Carditis	Inflammation of the heart's substance.
		ANEURISM	Aneurismus.
		Ossification of valves of the heart or arteries	Ossificatio.
		Rupture of heart, &c.	Laceratio.
		Syncope	Fainting.
		Angina pectoris	
		Phlebitis	Inflammation of the veins.
		Atrophy of heart, &c.	
		Hypertrophy of the left ventricle, &c.	
		Hydropericardium	Dropsy of the pericardium.
		DISEASE of the heart, arteries, veins, or lymphatics.	Morbus cordis, arteriarum, venarum.
	Of the Digestive Organs.	TEETHING	Dentitio.
		Salivation	Under Disease.
		GASTRITIS	Inflammation of the internal membranes of the stomach.
		ENTERITIS	———————— the bowels

			Synonymes, Provincial terms, &c.
Sporadic Diseases.	Mouth, Œsophagus. Stomach. Small Intestines. Colon. Rectum.	PERITONITIS	Inflammation of the peritoneum. (6)
		Tubercular peritonitis	—— ————, with tubercles.
		TABES MESENTER.	Mesenteric disease.
		ASCITES .	Dropsy of the peritoneum.
		CONSTIPATION	Obstipatio.
		Intussusception	Introversion of the bowels.
		Ileus . .	Iliac passion.
		Stricture of œsophagus, rectum, &c.	
		Colica pictonum	Painter's colic.
		COLIC .	Colica.
		HERNIA .	Rupture.
		ULCERATION of the stomach, &c.	Ulcus.
		WORMS .	Vermes. Worm fever, infatile remittent fever.
		Tape-worm .	Taenia.
		DISEASE (softening, &c.) of the stomach, and intestinal canal.	Morbus ventriculi, vel intestinorum.
		Dyspepsia .	Indigestion.
		Pyrosis .	Water brash.
		Piles .	Hæmorrhoids.
		Hematemesis .	Vomiting of blood.
	Pancreas.	DISEASE (inflammation, &c.) of pancreas.	Morbus pancreaticus.
	Liver, Gall-bladder	HEPATITIS .	Inflammation of the liver.
		JAUNDICE .	Icterus.
		DISEASE (abscess, enlargement, &c.) of liver.	Morb. hepaticus.
		Cirrhosis .	Drunkard's liver. Nutmeg liver.
		Gall stones .	Chololithus.
	Spleen.	Splenitis .	Inflammation of the spleen.
		DISEASE (enlargement, &c.) of the spleen.	
	Of the Urinary Organs. Kidneys, Ureters, Bladder, Urethra.	NEPHRITIS .	Inflammation of the kidneys.
		ISCHURIA .	Stoppage of the urine.
		DIABETES .	Diabetes mellitus. Sugar in the urine.
		GRANULAR DISEASE	Bright's disease.
		CYSTITIS .	Inflammation of the bladder.
		STONE .	Lithia. Calculus.
		STRICTURE of Ureth.	Ischuria urethralis.
		Strangury .	Stranguria.
		DISEASE of the kidneys or bladder.	Morbus renum, vesicae.
		Hæmaturia .	Bloody urine.
		Catarrh of the bladder	Catarrhus Vesicae.
	Of the Organs of Generation.	CHILDBED .	Partus
		Miscarriage .	Abortus
		Difficult labour .	
		Extra-uterine fetation	
		Rupture of uterus, &c.	
		Flooding .	Hæmorrhagia
		Puerperal convulsions	
		Puerperal fever .	Phlebitis (?)
		Phlegmasia dolens	Crural phlebitis (?)
		PARAMENIA .	Mismenstruation. Turn of life.
		Chlorosis .	Green sickness.
		OVARIAN DROPSY	Hydrops ovarii.
		DISEASE (in the male) of the testis, prostate gland, &c.	Morbus testis, &c.
		Orchitis .	Inflammation of the testis
		Paraphimosis .	
		DISEASE (in the female) of the uterus, ovaries, or vagina.	Morbus uteri, &c.
		Polypus uteri .	
		Inflammation of the breast.	

			Synonymes, Provincial terms, &c.
Sporadic Diseases.	*Of the Organs of Locomotion.* Joints, Bones, Ligaments, Tendons, Muscles.	ARTHRITIS	Inflammation of the joints.
		Periostitis	Inflammation of the periosteum.
		RHEUMATISM	Rheumatic fever.
		Chronic Rheumatism	Rheumatismus chronicus.
		DISEASE of the joints, bones, ligaments, tendons, or muscles.	Morbus articulorum, ossium, &c.
		Brittleness of bones	Fragilitas ossium.
		Softening of the bones	Mollities ossium.
		Caries	
		Necrosis	
		Lumbar abscess	Abscessus psoae.
		White swelling	Scrofulous inflammation of the joints.
	Of the Integumentary System. Skin, Cellular Tissue.	CARBUNCLE	Carbunculus.
		PHLEGMON	Inflammation of the cellular tissue.
		Whitlow	Paronychia.
		ULCER	Ulcus.
		FISTULA	
		DISEASE of the skin	Morbus cutaneus.
		Leprosy	Lepra.
		Purpura	Purples.
		Noli me tangere	
		Pompholyx	
		Impetigo	
		Scald-head	
		Elephantiasis	
	Of Uncertain Seat. (8)	Scorbutus	Scurvy. (7)
		HÆMORRH. (from—)	Bleeding. Hæmorrhagia.
		Epistaxis	Bleeding at the nose.
		Œdema	Dropsy of the extremities, &c.
		DROPSY.	General dropsy. Hydrops.
		ABSCESS of brain, &c.	(Under DISEASE of brain, &c.)
		,, liver.	
		MORTIFICATION of the foot, &c.	Gangraena.
		Dry gangrene	Gangraena senilis.
		Hospital gangrene	Gangraena nosocomialis.
		Spontan. combustion	Catacausis.
		CARCINOMA (of the breast, &c.)	
		Scirrhus.	
		Fungus hæmatodes.	
		Melanoma.	
		TUMOUR.	
		SCROFULA	King's evil.
		Bronchocele	Wen.
		Rickets	Rachitis
		Cachexia.	
		GOUT	Podagra.
		INTEMPERANCE.	
		ATROPHY	Wasting, Atrophia, Marasmus.
		STARVATION.	
		DEBILITY	Debilitas.
		MALFORMATION	Pseudomorphica.
		Cyanosis	Blue disease.
		Imperforate anus	Anus imperforatus.
		Spina bifida, &c.	
	OLD AGE,		Senectus.

DEATHS by VIOLENCE. (9)

(1) *Hives* is a synonyme of varicella; but in some of the northern counties it is understood to signify inflammation or mesenteric disease. Bowel hives, heart hives, also occur. Hives should not be employed in the registers.

(2) *Fit.*—This is an objectionable term; it is indiscriminately applied to epilepsy, convulsions, and apoplexy, in different parts of the country.

(3) *Inflammation of the chest* is an indefinite term.

(4) *Hæmoptysis* is sometimes improperly registered "Rupture of a blood-vessel."

(5) *Decline* is improperly applied to phthisis in some districts.

(6) *Inflammation of the bowels* is an objectionable term; as it does not distinguish peritonitis from enteritis. Some writers improperly apply "enteritis" to inflammation of the part of the peritoneum over the bowels. *Gastro-enteritis* in the Abstract includes *Enteritis* and *Gastritis*.

(7) *Scurvy* is improperly applied to several cutaneous diseases.

(8) *Inflammation, abscess, tumour, &c.* occur in the tables. The *part affected* has not been named in these cases, although in inflammation the omission renders the term almost unintelligible. *Sudden death* has been used in the Abstract as a general head for all the vague verdicts of coroners' inquests. If the cause of death were ascertained at the inquests, such a term would scarcely be needed.

(9) *Deaths by violence.*—Coroners' inquests are held in all cases that fall under this head; and the Registration Act directs the registrar to make the entry according to "the finding of the jury."

At the end of the whole are placed deaths by old age and violence, and under the latter head are included all fatal accidents, injuries, different modes of suffocation, death by poisons, by quack medicines, by the bites of animals, and similar catastrophes.

According to the method observed in this list, Mr Farr has classified in Table A., in the First Report, a view of the numbers cut off by different diseases throughout England and Wales, from the 1st of July 1837, to the 31st December of the same year, and he gives a general view of the comparative frequency of each tribe of diseases; and a similarly classified list is given for 1838 in A. of the Second Report. In the survey which we now propose to make, we shall also consider how far the method afforded by the table is a convenient and correct method of classification for statistical purposes.

The total number of deaths during this half-year, when it was supposed the mortality was low, amounted to 148,901, of which 75,159 were males, and 73,542 females. The cause of death was specified in 141,607 cases, leaving 7094, or 4.8 per cent. unaccounted for. The total amount of deaths during the year 1838 was 342,529, of which 175,046 were among males, and 167,485 among females. If the total amount be halved, it gives 171,264 for the half-year, which is 22,563 more in the half-year than in 1837, or about 45,000 more in the whole year. Mr Farr estimates the population of England and Wales to be on the 1st October 1837, 15,268,056; or 7,439,128 males, and 7,828,933 females. He finds, after making the requisite deductions and corrections, and dividing the population by 2, in order to reduce it to the state of the half-year's mortality, that the rate of mortality during that half-year was 2.08 per cent. among males, 1.97 among females, or 2.02 per cent. for both sexes,—1 in 48 for males, 1 in 51 for males, or 1 in 49 for both sexes. This is a rate of mortality much lower than in 1838, when, according to the amount already given among a population of 7,668,265 males, and 7,885,615 females, or 15,553,860 of both sexes, the mortality was 2.28 per cent. or 1 in 44 among males, 2.12 or 1 in 47 among females, and 2.20, or 1 in 45 in both sexes. It results, therefore, that the mortality in 1838 was much more considerable than in 1837, by 1 in 4. It becomes next a matter of consideration to ascertain, if possible, in what division or section of sickliness this mortality was increased in 1838 above that of 1837.

It appears, in the *first* place, that among the whole amount of 148,701 for the latter half of 1837, 32,537 were caused by epidemics, and diseases reputed epidemic or contagious. This shows that rather more than one-fourth of the mortality of England and Wales was caused in 1837 by epidemic distempers, as small-pox, measles, scarlet fever, and continued fever. Small-pox destroyed 5811, or about one-sixth; measles, 4732, or one-seventh; hooping-cough, 3044, or about one-tenth; diarrhœa, 2755, or about one-twelfth, and scarlet fever, 2520, or about one-thirteenth.

This mortality was almost confined to children, excepting in the cases of small-pox, in which a considerable mortality appears to have taken place above the age of puberty. The highest amount of mortality of this class was that occasioned by typhous fever, which amounted to 4439 among males, and 4608 among females, producing among the two sexes a mortality of 9047, or rather less than one-third. It appears that typhus and small-pox were the great epidemic disorders.

In the *second* place, it appears, that in 1838, 32,321 males and 33,556 females died in consequence of diseases of the epidemic class; or 67,877 of both sexes. Supposing that the half of this amount took place during the half year, viz. 33,938, this is a little more numerically than the deaths (32,537) produced by the same class of distempers in 1837. But when the larger amount of the population in 1838 is considered, the mortality in proportion is smaller from the epidemic class of distempers. It appears accordingly that, while the mean annual mortality in 1837 was 4.6 per 1000, or 4.7 per 1000 of males, and 4.5 per 1000 of females, it was in 1838 only 4.5 per 1000, or 4.6 per 1000 of males, and 4.4 per 1000 of females.

It appears in the *third* place, that the epidemic distempers did not amount to more than one-fifth of the whole mortality in 1838 ($\frac{342529}{67877}$), whereas in 1837 it was fully one-fourth.

In order to form a correct idea of the comparative mortality caused by each kind of epidemic distemper in 1837 and 1838 respectively, Mr Farr gives tables exhibiting the numbers dead in a mean population of 1,000,000 of each sex. From this it appears, that, while small-pox and typhous fever are at the head of the list for their destructive powers, the former among children, the latter among adults; yet typhous fever was in a small degree less fatal in 1838 than it had been in 1837, having caused 125 deaths in 100,000 in 1838, and 129 in the same number in 1837. But small-pox conversely was greatly more fatal in 1838 than in 1837, having caused in 1847, 83 deaths in 100,000, and 108 deaths, or one-fourth more in 1838. It appears, indeed, that an epidemic of small-pox prevailed in England and Wales in the latter end of 1837 and the whole of 1838, and destroyed, especially in the latter year, a very great number of persons. It is stated by Mr Farr to have destroyed more than 30,819 persons.

The next great destroyers of the human race in 1838, were hooping-cough, (60 per 100,000); measles, (43.1 per 100,000); scarlet fever, (38.7 per 100,000); croup, (29.8 per 100,000); diarrhœa, (16 per 100,000); rose, (8 per 100,000); and thrush in the order now specified. In 1837, the order is as follows, after fever and small-pox, measles, (67 per 100,000); hooping-cough, (43); diarrhœa, (39); scarlet fever, (36); croup, (23); thrush, (10), and dysentery, (9).

The total amount of deaths by sporadic diseases was 116,164, or about three-fourths of the whole. Of this amount of mortality, by far the largest proportion ranges itself under the head of diseases of the organs of respiration, being 38,522, or one-third, 27 per cent. of the total number. The next under that of diseases of the nervous system, being 21,852, or one-fifth, 15 per cent. of the whole number. The next is referred to diseases of uncertain seat, amounting to 16,207, or about one-seventh. The next, diseases of the digestive organs, amounting to 9850, or one-twelfth. The next, affections of the organs of circulation, amounting to 1596, or one-seventieth part. The next, diseases of the organs of generation, amounting to 1498; and last of all come such diseases of the locomotive organs, as rheumatism, gout, and necrosis; diseases of the urinary organs, and diseases of the skin and cellular tissue, in the order now specified.

In 1838 the total amount of deaths by diseases of the sporadic class was 274,652. If this be divided by 2, in order to render it susceptible of comparison with the mortality of the latter half of 1837, it gives 137,326, which is 21,162 more than the mortality caused by the same class of diseases in 1837. When compared with the existing population of each year, it appears that while the mortality by sporadic diseases in 1837 was only 1 in 65, that in 1838 was 1 in 56, which shows that a real increase in the mortality was caused by this class of distempers during that year.

It appears that, of the total number of sporadic diseases, 274,652, in the year 1838, 90,823, or one-third of the whole, was caused by diseases of the organs of respiration, nearly 60,000 alone being caused by pulmonary consumption; 49,704 by various disorders of the brain and spinal marrow or their investments; 44,232 by diseases of uncertain seat; and 19,306 by diseases of the digestive organs.

The short estimate of the great outlets of human life, as they may be termed, by sporadic diseases, shows that the destruction by pulmonary disorders was very nearly the same in the latter half of 1837 and during 1838; that the diseases of the nervous system were rather more numerous in 1837 than in 1838; that the diseases of uncertain seat were considerably more numerous, forming at least one-sixth of the whole tribe of sporadic diseases; while those of the digestive organs were rather less numerous, being somewhat less than one-fourteenth of the whole.

We shall now consider shortly the comparative mortality of the two periods, under the respective heads of the different diseases to which they are referred, and we propose at the same time to inquire how far the present method of classification is calculated to fulfil the purpose proposed, and to what extent it is calculated to insure against errors and fallacies, both in returns, and in the deductions formed from them.

We begin first, though not in the order of the author, with the division of Sporadic Disorders.

Among the diseases of the nervous system, viz. those affecting chiefly the brain, *Cerebellum*, and spinal chord, and their investing membranes, and which family forms the first in the list, though not the most numerous, convulsions constitute by far the largest proportion, amounting to 10,729, or nearly one-half of the whole number. Next to these comes the head of *hydrocephalus*, or water in the brain, amounting to 3570, or about one-sixth of the whole. It is extremely probable, that, if we consider these two disorders in a pathological point of view, they must be regarded as closely allied, or rather as proceeding from one common cause. Convulsions are merely external symptoms of congestive or inflammatory irritation of the brain and its membranes,—often of the spinal chord and its membranes. Hydrocephalus is the ultimate effect of an irritation of the same kind, milder in character and slower in progress. The cause which produces convulsions very often terminates in hydrocephalus; and of the justice of this view, there is, among other proofs, a very clear one in that peculiar form of hydrocephalus named water-stroke (*Wasserschlag*), which is very sudden in approach, and which passes over its several stages to the fatal termination in a very short time. If these two heads be conjoined they would constitute the large amount of 14,399, or two-thirds of the whole amount of diseases affecting the cerebro-spinal axis.

In the year 1838 the mortality by convulsions is not less than 26,047, which is much more than one-half of the mortality by this order of diseases, and consequently much more than that by the same cause in the previous year. If the two heads of hydrocephalus and convulsions be conjoined in this as in the former case, the amount will be 33,719, or rather more than two-thirds of the whole of this class of disorders. It is not unimportant to observe, that, in these two families of nervous diseases, males are destroyed more numerously than females, in nearly the same ratio.

Mr Farr is of opinion that, as convulsions are a frequent termination of all infantile diseases, and are subject to great variations, they probably include several diseases undeveloped, or are the result of diseases of the epidemic class. This may be the fact; but, if convulsions are to be regarded as the effect of any of the tribes of epidemic disease, there must be great risk that many cases are registered or returned under two heads at least, both the primary epidemic disease, and the secondary or symptomatic disorder; and in making such returns, it is manifest that the greatest care will not always be sufficient to avoid this error, and consequently a fertile source of confusion and incorrect results.

The undeveloped or imperfectly developed diseases of which they are the result are either idiopathic meningitis, or that irritation of the cerebro-spinal axis which depends on gastro-enteric disorder.

It seems singular that Mr Farr should arrange *cephalitis*, or inflammation of the substance of the brain, with hydrocephalus and convulsions, as often a disease incident to young children; and nothing can place in a clearer light the error of admitting in the same list a disease designated by its pathological character, and one designated only by symptoms. If the modern doctrines regarding cephalitis be well founded, that malady, understood as cerebral inflammation, is the cause of palsy and of apoplexy, in some instances of convulsions, and in some of insanity; and it corresponds in this view partly with apoplexy, and palsy, epilepsy, and insanity, and partly with the vague and not very useful general head of disease. It appears, however, that Mr Farr understands by cephalitis, inflammation of the substance of the brain taking place in the young, viz. phrenitis. But even with this explanation, a considerable source of confusion is added to that already noticed in separating hydrocephalus and convulsions. If it is to be employed to designate inflammation of the brain of this variety, it is impossible to understand how it is to be distinguished from hydrocephalus, or rather meningitis; and it appears to us that cephalitis, hydrocephalus, and convulsions ought all to have been referred to the single head of meningitis, or inflammation of the membranes of the brain.

This view of the nosological nature of cephalitis, nevertheless, is favoured by no modern author excepting Charpentier, who maintains, in opposition to what is seen daily in inspecting the brains of children destroyed by this disorder, that hydrocephalus is the effect of inflammation of the substance of the brain. So far as our own knowledge of the subject goes, we think that there is abundant evidence to show that the *cephalitis* of Mr Farr is the same as *meningitis*, and that water in the brain is a pathological effect, while convulsions are a symptomatic effect of this disorder. If, therefore, the numbers of the other two heads be increased by that of cephalitis, it will raise this tribe of cerebral or meningeal to the number of 35,897.

A remark, partly suggested by Mr Farr himself, would lead us to place teething, when terminating fatally and viewed as a cause of death, under the same head in a large proportion of cases. The numbers destroyed during this process in 1838 were 2294 male children, and 2110 female children, or 4404 of both sexes. Teething, when terminating fatally, generally does so by reacting on the brain. This, however, we may notice afterwards.

Apoplexy and palsy, which may also be regarded as symptoms of the same state of the brain, when the latter terminates fatally, come next in frequency and importance, the former destroying 2711, and the latter 2039, or both together 4750, or considerably more than one-fifth of the number who die from this class of diseases. These diseases affect chiefly adults. But it is curious to observe that, while a larger proportion of males are destroyed by apoplexy than of females, the number of females destroyed by palsy (1052), is a little more than that of males (987).

In 1838 these two families of disease destroyed 10,605, viz. apoplexy 5630, and palsy 4975; and it is here to be observed, that, while a much larger number of males is destroyed by apoplexy (2938) than of females (2692), and a much greater number of males is destroyed by apoplexy than by palsy (2307), the number of females destroyed by apoplexy (2692), and by palsy (2668), is much the same.

Epilepsy appears to have produced rather greater mortality in 1837 than in 1838, the mortality for the latter half of the former year being 570, and for the whole of 1838, 1093, or for the half year 546½, which is smaller, considering the increased number of the population.

Insanity had also caused a much smaller mortality in 1838 than in 1837. In the latter half of 1837 the mortality from this cause was 285. In 1838 it was for the whole year only 167, or 183.5 for the half year. Insanity, it may be observed, does not usually prove fatal of itself; and it is rather by being associated with meningeal effusion, or meningeal apoplexy, or with cerebral hemorrhage, or softening, or some organic change, or by being complicated by pulmonary or intestinal disorder, that life is terminated.

Rather a large cypher is found under the general head of Disease in both years. As the number is 764 for the latter half of 1837, and 1407 for the whole of 1838, and as this is understood to embrace softening of the brain, and perhaps many organic changes in that organ or its envelopes, it is most reasonable to think, that, had the deaths under this head been duly considered and inquired into, they would have classed themselves under the head of apoplexy and palsy, or encephalitis.

We have taken the liberty of pointing out several defects in the classification of the diseases of the nervous system; and it appears to us, that, unless the arrangement of this division be very much altered indeed, it will not be practically useful in forming tables of the prevalence or mortality of the different diseases. We deny not that it is easier to point out than to supply defects, and we are unwilling to say how these may be supplied, or the errors rectified. But it is clear that, as it at present stands, the list is likely to be the source of many fallacies.

We shall see presently whether the section of diseases of the respiratory organs is more free from objection.

Under the name of Quinsey we find 289 deaths in the latter half of 1837, and 432 in 1838. Though this cannot be said to be a high mortality, being about 1-200th part of the section, it is still higher than a disease so trifling as quinsey usually is ought to be; and this compels us to express the opinion, that various affections of the throat, perhaps of a more formidable nature, have been arranged under this denomination.

Bronchitis caused in the latter half of 1837 a mortality of 460, and in 1838 of 2067, being more than double the mortality of the previous year, which may have been about 920. A proportionate increase in other diseases of the lungs appears at the same time to have taken place in the course of 1838, for pneumonia, which had been the cause of death to 5824 persons in six months of 1837, or at the rate of 11,648 during the year, destroyed, in 1838, 17,999 persons, or 6351 more than the previous year, which is a much larger proportion than it should have done, considering the increase in the population. The accuracy of this statement is shown still more clearly by comparing the proportionate mortality from these diseases in 1837 and 1838, as exhibited in Tables B. B. in the two reports. It there appears that, in 1837, bronchitis destroyed 66 in 1,000,000, and pneumonia 834 in the same number; whereas the former disease destroyed, in 1838, 138 persons in 1,000,000, and pneumonia 1201.

In considering the mortality induced by this table of disorders, it appears to us that a great mistake has been committed, and a fertile source of fallacy is furnished, in excluding from it the deaths attributed to croup. These Mr Farr places, according to his nosological arrangement, among the distempers reputed epidemic, endemic, or contagious; and their frequency is testified by the fact, that, in the latter half of 1837, this disorder is computed to have destroyed 1655 children, and, in 1838, 4463. What circumstance can entitle this distemper to the character of epidemic or endemic, for contagious it can never be considered, any more than bronchitis or pneumonia, we confess it is difficult to perceive. We are aware that it has been represented to prevail in low wet localities, near the sea-coast, or outlets of rivers; but to this it must be answered, that bronchitis and pneumonia prevail in the same localities, that croup appears in these districts chiefly in the winter and the spring, and that it also prevails in inland and elevated situations during the winter season. In truth, croup is a disease more of season than of locality, and every thing in its history and origin shows its intimate connection between bronchial disorders and pulmonary affections. This arrangement is so much the more extraordinary, that Mr Farr distinguishes between *laryngitis* and croup; a circumstance not always attended to by practitioners. In the propriety of the distinction as a pathological principle we entirely agree; but when we view these disorders in reference to their etiology, it is quite clear that croup ought to be placed between laryngitis on the one hand, and bronchitis on the other. It is produced in the same mode; it arises from the same exciting causes, viz. exposure to cold and moisture; and it not unfrequently is associated with one or other, if not both of these maladies. Viewed as a tracheal or tracheo-bronchial inflammation, in short, no good pathologist can doubt that its proper place in a statistical nosology is in the present section, rather than in that of distempers reputed epidemic or endemic.

It must seem singular, that, at the present day, it should be proposed to place such symptomatic changes as hydrothorax and asthma among the list of diseases of the organs of respiration. Morbid anatomy as well as observation concur in showing, that the effusion called hydrothorax, or water in the chest, by the old nosologists, is never idiopathic, but is the effect in general of disease of the heart, hepatization or other disease of the lungs, tumours of the larger vessels, or, when conjoined with effusion into the other cavities, is the effect of disease of the liver, or disease of the kidney. From whichever of these sources it proceeds, it is clear that it has no title whatever to a place in this division; and it ought to have been arranged either under the head of diseases of the organs of circulation, or disease of the liver, or disease of the kidneys. Yet to this cause are ascribed in the latter half of 1837 995 deaths, of which 557 took place in males, and 438 in females; and, in 1838, 2306 deaths in 1295 males, and 1011 females; which is a larger proportion than might have been expected from the increase of the population.

The objections against placing a complaint so anomalous in its causes as asthma under this head are still stronger. The deaths ascribed to this cause in the latter half of 1837 are 1764; and in

1838 they amounted to 5745, or 2872.5 for half year. It is utterly impossible that numbers so very unlike can depend upon the same cause, and there must be here some very great fallacy. Can the source of this fallacy not be fancied in the use of a term so vague and so erroneously applied as asthma? This term generally means several things totally different, according as it is used to denote a complaint in the living or a cause of death. When used in the former sense it has been known to be applied in all the following senses; 1. to denote dry chronic bronchitis with emphysema of the lungs; 2. the breathlessness of excessive corpulence; 3. fits of breathlessness occurring in the gouty; the breathlessness caused by tumours in the breast, or compressing the windpipe or large vessels; the breathlessness from encephaloid tumours in the pleura; the breathlessness from tubercular disease of the lungs, either in the early stage, the confirmed and excavated stage, or the stage of healing; the breathlessness of chronic laryngitis and ulceration of the windpipe; and the breathlessness caused by hypertrophy of the heart, effusion of fluid within the pericardium, and effusion of fluid within the pleura. Considered as a cause of death, the term asthma is applied sometimes to various diseases of the heart and large vessels, as hypertrophy, valvular disease, and aneurism of the aorta innominata, or the subclavian artery, or to their effects,—as fluid effused within the pleura and pericardium, and sometimes to chronic bronchitis with pulmonary emphysema, or to various malformations of the chest. It can scarcely be doubted that, as it is in the latter point of view only that it can be admitted into the present list, the large amount of deaths ascribed to it must have depended upon causes very different from those which are here supposed to exist. Of this large amount of deaths, then, part ought to have come under the head of chronic bronchial disease, and part under the head of disease of the heart, in the following section.

A considerable number of deaths is placed under the general head of disease of the organs of respiration. In the latter end of 1837 this amounted to 1176, and in 1838 to 2568. It is not unlikely that many of these deaths belong to chronic bronchitis, emphysema, consumption, and perhaps organic changes in the lungs. It appears that, under this head, are ranged such lesions as ulceration of the larynx, contraction of the windpipe, pulmonary apoplexy, and gangrene of the lungs.

Among the diseases of the organs of circulation, the heaviest article is that of Disease, to which 1596 deaths (834 males, 648 females,) are ascribed in the latter half of 1837, and 3562 (1870 1449 females,) in 1838. There is to this no great objection, as it is not always possible without dissection to distinguish the different forms of disease of the heart, and for all statistical purposes the general denomination of disease of the heart is perhaps sufficiently precise, including under this head mostly hypertrophy with or without disease of the valves, and that variety of hypertrophy of the right ventricle towards its base, which is very generally associated with disease of the lungs.

The only observation which we would here add is that which has been already made, that this article would require to be augmented by the addition of a large proportion of the deaths referred to asthma and hydrothorax, if these deaths have not been referred to both heads.

Under the extensive and multifarious, though not numerous, section of diseases of the digestive organs, are placed several diseases which undoubtedly belong to that head. But we do not find there all the diseases which we should have expected to see; such, for instance, as diarrhœa, dysentery, and cholera; and when we look for these affections, we find them with thrush under the section of epidemic and endemic distempers. Now we cannot help thinking that this method of arrangement and distinction is another example of the erroneous results proceeding from the practice of designating diseases at one time by symptoms and at another by their pathological characters. The same disease may in this manner be divided into three or four different affections, to the great confusion of the report, the perplexity of the reader, and the annihilation of all accuracy and precision. This, we trust, will appear in a pretty clear light from the following remarks.

The first article under the head of the diseases of the digestive organs is that of teething, to which, in the latter half of 1837, 1903 deaths (viz. 998 males and 905 females) are ascribed; and in 1838 the large number of 4404, (2294 males, and 2110 females.) Now what is meant by the term teething, understood as a cause of death? Does it mean any single disease, and what is that single disease; or does it include several? Dr Burns states, that, in irritable and delicate children, "the stomach and bowels are rendered irritable," producing griping pains; and in other cases severe bowel complaints or fever, or spasmodic cough, or convulsions take place, or the skin is affected with eruptions; or "sometimes the tongue and mouth become aphthous."*

Dentition, though generally a process of health, may, according to Dr Evanson, give rise to sundry nervous symptoms; to aphthæ and febrile irritation; to convulsive motions and sudden death as if by apoplexy; or incurable palsy; to spasmodic contraction and incurvation of the flexors of the thumbs and toes; to spasm of the glottis or *laryngismus stridulus*; to purging or vomiting, or both, with acidity or sour smell of the discharges; and to cholera. There is indeed scarcely a disorder of the alimentary canal, of the skin, or of the nervous system, which has not by different authors been ascribed to the process of dentition. It is chiefly, however, with those that may terminate fatally that we have at present to do. The principal of these are the disorders of the alimentary canal indicated by watery or greenish diarrhœa, sometimes terminating in softening of the stomach, sometimes in fatal cholera, sometimes in ulceration of the mucous membrane of the bowels, sometimes in the anomalous disorder called weaning-brash by Dr Cheyne, and which seems often to depend on chronic inflammation and ulceration of the aggregated intestinal follicles; aphthous inflammation, irritation, or meningeal inflammation terminating in hydrocephalus or convulsions; crowing inspiration, *(laryngismus stridulus)*, with the corresponding disorder of the flexors of the thumbs and toes.

---

* Book iv. Chap. iii.

From the facts now adduced it must follow, either that the article named teething ought to be distinguished into several articles referable to different heads, or, if it be allowed to remain in its present position, that it ought to be increased by the deaths ascribed to thrush, to diarrhœa, and to cholera in the first section, and those ascribed to convulsions and hydrocephalus in the second. At the most, what security is there that deaths returned as dependent on teething may not have been returned under the heads of thrush, diarrhœa, hydrocephalus, or convulsions? Upon the whole, we are disposed to think that the best plan would be to expunge the article dentition as a cause of death altogether, and to make the deaths taking place during this process be returned under one or other of the diseases which are known to be most fatal during the process. Under all circumstances, we respectfully submit that, if teething be referred to the head of disorders of the alimentary canal, by as strong a title do thrush, diarrhœa, and cholera merit this position.

On the subject of thrush we observe, that there are manifestly two if not several varieties of this distemper; one mild, and rarely fatal; another severe, and often unmanageable and fatal. In this country it is rarely a disease of itself or necessarily fatal, and when death takes place, it is not in consequence of the thrush, but in consequence of the gastric or intestinal disorder of which the thrush is a mere symptom. In the latter half of 1837, 707 deaths, and in 1838, 1090 deaths are ascribed to this cause. We must be permitted to express our doubt of the correctness of this return; and to state most confidently, that, had the cases been carefully considered, they would have ranged themselves either under diphtheral inflammation, or *gastritis*, or *gastro-enteritis mucosa*.

The severe and ofttimes fatal form of thrush (Le Muguet, Le Millet,) found to prevail in certain wet districts in France and Belgium, is equally a symptomatic effect of various disorders of the gastro-intestinal membrane. Though the latter form of the disease may be thus considered as endemic or dependent on local physical causes, yet there seems little reason for removing this disorder from the list of affections of the alimentary canal.

It further appears that thrush includes the peculiar gangrenous affection called water-canker or *cancrum oris*,—a malady which, so far as we have seen, cannot be regarded as epidemic or endemic, but rather dependent on some malignant though unknown state of the constitution.

Diarrhœa, which occupies a place next to thrush in the first section, is in like manner to be viewed as a symptom of irritation or inflammation of the intestinal mucous membrane and its follicles. It can almost never be regarded as an independent or primary disorder; and even in those instances where it is said to be the effect of cold or a surfeit, or improper article of diet, what reason have we to imagine that it is any thing but a symptom of the irritative congestion then temporarily excited. In all other instances, and especially when it continues for more than two or three days, it must be viewed as a symptom of inflammation of the mucous membrane, (*enteritis mucosa*, Cullen,) *enteria*. This we have had occasion to explain so fully, and to demonstrate so clearly in former volumes, (xxi.) that we think it unnecessary now to dwell on the subject. We merely add, that, when we find *gastro-enteritis* among the sporadic diseases of the present list, and diarrhœa among the endemic and epidemic disorders, we are compelled to say that confusion and manifold errors can scarcely be avoided. Is not *gastro-enteritis* a cause of diarrhœa; and is not diarrhœa one of the most common and essential symptoms of *gastro-enteritis?* It is clear, at all events, that, viewed as a cause of death, diarrhœa can never be considered in any other light than as a symptom of inflammation or ulceration of the intestinal mucous membrane. Upon the whole, we think it is quite impossible to resist the inference, that the term diarrhœa ought either to be confined to those slight instances of intestinal discharge, in which there is mere irritation of the mucous membrane of the bowels for 26 or 36 hours; and then it is to be remarked it is never a cause of death; or that it ought to be expunged altogether from the list, and the deaths ascribed to it ought to be referred to the head of *gastro-enteritis*, or *enteritis mucosa.*

With regard to dysentery, all that we have to say is, that it must be regarded as decidedly a symptom and effect of inflammation of the mucous membrane of the colon in different parts of its course; and if, therefore, it is to be received as an article in the list at all, it ought to come along with or immediately after *enteritis mucosa.*

Gastro-enteritis, that is, inflammation of the mucous membrane of the stomach and bowels, forms the second article under this head;

and to it are ascribed, in the latter half of 1837, 3396 deaths, (1710 males, 1686 females,) and in 1838, 6061, (3139 males, 2922 females,) which, if divided by 2, so as to render it comparable to the same annual period of 1837, gives 30,305, which is rather a smaller mortality than in the previous year. It is, nevertheless, not improbable, that, as the same annual period of 1837, included the autumnal season, always a heavy period for deaths from intestinal disorders, had the circuit of the whole been taken it would have brought the amount of the mortality of this article nearly to that of the subsequent year.

Tabes mesenterica, to which 437 deaths are ascribed in the latter half of 1837, and 724 in 1838, must be regarded as the alternate termination of inflammation and ulceration of the mucous tissue of the bowels. Excluding anomalous cases of intestinal and abdominal diseases, often referred to this head, the strumous enlargement and softening remarked as the anatomical character of this malady, is the effect of previous ulceration of the muciparous follicles of the ileal glands. Perhaps, therefore, this article might be conjoined with gastro-enteritis, though upon this we do not wish to dwell strongly.

The head of ulceration, however, might surely be referred either to tabes or to gastro-enteritis. All ulceration, we presume, to be the effect of inflammation; and if it were intestinal ulceration, there is no doubt that it would cause tabes as readily and certainly as ulceration of the follicles.

In the Report for the latter half of 1837, colic and constipation form separate heads; and to the latter 461 deaths are ascribed, including those from intussusception, and also ileus and stricture. In that for 1838, a considerable change is introduced in the two articles now mentioned. Colic or ileus is made one head, producing 619 deaths, viz. 345 males, and 274 females. The article constipation is entirely expunged, and in its place is substituted that of intussusception, producing 238 deaths, viz. 136 males and 102 females. This must be regarded as an improvement upon the method of 1837, in the first Report, in so far as the denominations are more precise, and free from ambiguity. Constipation is decidedly an improper epithet, as it is a mere effect of several different morbid states of the bowels, or their appendages.

In the Report for 1838, we find hæmatemesis introduced as the cause of 111 deaths, 65 among males, and 46 among females. This must be a considerable source of perplexity to many readers, as we must say it is to us. Hæmatemesis is often a mere symptom or a vicarious hemorrhage; but as such, however alarming, it is almost never fatal. In such circumstances, also, it is more frequent in females than in males; and indeed it is rarely seen in the latter, unless as the result of organic changes in the stomach, the intestinal tube, the liver, or the spleen. In the present case, however, it is a fatal disorder, and it is greatly more common in males, in the ratio of 13 to 9, than in females. This cannot, therefore, be the disease described by nosological authors as vomiting of blood. Is it likely that it belongs to the head of Melaena, which is certainly more commonly fatal? But to this again must be opposed the objection, that no mention is made of the intestinal discharges, or of their character. The only construction that can be put upon this article and its fatality is, that the discharge of blood must have arisen from aneurism of some of the arteries of the stomach or the neighbouring organs, or ulceration of the stomach, or fungous, that is encephalomatous disease of the stomach or neighbouring organs. It is quite clear that on this subject some more precise information is required.

It must not be forgotten that, in the latter half of 1837 only 30 deaths under the general name of disease are ascribed to haematemesis, in which the same preponderance in the number of males (18) over that of females (12) is remarked; a clear proof that it must have proceeded from some great organic change in the tissues or blood-vessels of the stomach. This would make at the rate of 60 deaths from this cause in the whole year; and it is still to be observed, that the number of deaths assigned to this cause, viz. 112, nearly double, is unusually great.

The general article of Disease is rather heavy in both years; being in the latter half of 1837, 853, and in 1838, 1385, that is, about 1400,—an annual average not accounted for in precise or specific terms.

It appears that, in the Report for 1838, disease included "cases of chronic *enteritis*, *gastritis*, and dyspepsia, as well as some malignant diseases." But it may well be asked, when was dyspepsia, as a mere dynamic disorder, ever of itself fatal? It may safely be said, that, if cases presenting dyspeptic symptoms terminated fatally, most certainly these dyspeptic symptoms were the effect of organic disease in the stomach, sometimes simple inflammatory or ulcerative, sometimes malignant, for instance carcinomatous or encephalomatous.

Under the section Diseases of the Urinary Organs are one or two articles which must perplex not a little in the present state of pathological knowledge. To granular disease, which forms one article, are assigned only 3 deaths;—2 males and 1 female. In the nosological table, this is distinctly explained as Bright's disease. Now it is absolutely impossible that, among a general amount of 816 deaths, there could be so small a number produced by granular disease of the kidney as 3. At the end of the list, however, under the head Disease are placed 309 deaths, viz. 262 male, and 47 female; and we are also told in the general appendix, Table B., that Bright's disease is registered disease of the kidney.

This seems a singular and extraordinary piece of confusion. Of the same disease apparently two different articles are made, one with a distinct specific denomination, the other with a general and vague epithet, so as to leave the reader very much at a loss what to understand. It is perhaps unnecessary to say, that even the number of deaths allotted to this cause is so extraordinarily small, that we think no one who knows anything of the extensive prevalence of that disease can place in it the slightest confidence.

In the table for 1838, the article granular disease is expunged; but we find under the head of Disease 710 cases, 578 being males, and 132 females. These numbers certainly make a nearer approach to the amount of deaths which assuredly take place in reality from granular disease of the cortical part of the kidney; but nowhere do we find any positive information that these 710 deaths are really ascribed to this cause. Conversely, when we direct attention to the large amount of deaths ascribed to dropsy, 5584 in the latter half of 1837, and 12,342, 5170 among males, and 7172 among females in 1838, the conclusion is inevitable, that a very considerable proportion of these deaths must have been the result of disease of the kidney. To this subject, however, we shall afterwards recur.

In the section on diseases of the generative organs in the latter half of 1837, 1265 deaths, all of course among females, are ascribed to child-bed, and of these the greater proportion died of puerperal fever. In 1838, this condition was not less productive of mortality, having caused 2811 deaths, which is rather a larger amount proportionably to the population than in 1837. The mortality, indeed, in 1837 was 4 per 1000 cases, but in 1838, 5 per 1000 cases. The great majority of this mortality is ascribed in 1837 to puerperal fever; in 1838, it seems to be ascribed also in some degree to miscarriage. There is no doubt of the great mortality of puerperal fever, chiefly by reason of the complex and severe lesions which take place in that condition; and it is very likely that the estimate by Dr Ferguson, that seven-eighths of the total mortality in child-bed is caused by this disease, is not far from the truth. The annual mortality from this cause is at present 2800 females, or 5 in every 1000 cases.

A very large increase in the amount of mortality from rheumatism and disease of the organs of locomotion had taken place in 1838. In the latter half of 1837, 437 deaths are ascribed to rheumatism, and 477 to disease, understanding by this, caries, necrosis, fragility of bones, softening of the bones, lumbar abscess and disease of the joints. In 1838, 1030 deaths are ascribed to the former cause, and 1056 to the latter, making an increase, if these numbers be halved, (515, 528) of from 1.3 to 1.4 in 10,000

The section of diseases of uncertain seat forms a large and numerous article;—not fewer than 16,207 in the latter half of 1837, and 44,212 in 1838; and of this by much the largest proportion is ascribed to inflammation (2337, 5816); dropsy (5584, 12342); debility (2406, 12634); and sudden deaths (1053, 3012).

We begin by observing, that either a singular error is committed, or we do not understand the statement made by Mr Farr, as to the deaths ascribed to inflammation in 1837. In the table it appears that to this cause 2337 deaths are ascribed. It is stated, however, by Mr Farr, in the general summary, that 14,105 deaths were ascribed to inflammations. This discordance we do not understand; and the statement seems further totally inconsistent with the fact, that the whole amount of deaths ascribed to diseases of indeterminate seat, were only 16,207, which is about 2000 more than the inflammations of indeterminate seat. The number also seems absolute, and not proportional.

It is singular that, in so large an amount of inflammatory disorders, the seats of the inflammation could not be ascertained and specified. It is manifest that this proceeds from the neglect of those who made the returns in the country; and it is to be hoped that means may be taken to render this head less numerous and more precise.

Hemorrhage, to which the deaths of 369 males and 217 females, in the latter half of 1837, and 710 males, and 488 females, in 1838, are ascribed, is not a very good title for the alleged cause; for it is almost uniformly a symptom or effect either of disease of the lungs or heart, or of aneurism, or sometimes of disease of the liver or intestinal tube.

To the large amount of deaths referred in both years to the article of dropsy, we have already partly adverted; and we are more thoroughly convinced than ever, that, as a cause of death, this ought altogether to be changed. Dropsy is not itself, correctly speaking, a disease; but it is invariably the result of some diseased state of several different organs. It may be the effect either of disease of the heart, disease of the lungs, disease of the liver, or disease of the kidneys; and instead of the large amount of deaths assigned to this head, the numbers ought to have been ascertained and allotted to each. No doubt can be entertained, that a considerable proportion of the deaths here assigned to dropsy ought to have been ascribed to disease of the kidney; some, perhaps, to disease of the heart; some to disease of the liver; and not a few to disease of the kidneys and the heart. It could not be very difficult to settle this point as to disease of the kidney; for any practitioner might easily obtain the urine even from the dead body, and ascertain its coagulable or uncoagulable characters, and even its density, without much trouble. Mr Farr would render these returns infinitely more accurate, by impressing on his correspondents the necessity of this mode of procedure, and thus enable himself to construct tables more accurate and useful than the present.

We are also very much disposed to doubt the propriety of the article Debility, as a cause of death ; for though unquestionably death very often takes place in consequence of apparent debility, yet when careful inquiry is made, it very generally turns out that the debility was the result of some great change in one or more of the organs more or less essential to life. We have seen ascribed to debility, deaths produced by disease of the lungs, disease of the heart, and disease of the kidney. In short, the latter disease assumes so many different characters, that it is often not suspected till disclosed by dissection. Thus it is common to say, that a person in delicate health went on very well till his stomach became affected, till he lost appetite, and began to reject and loathe foad. It is very rarely remembered, however, that this is a very common result of disease of the kidney, and when it has once commenced, continues more or less urgently through the whole course to the termination of the disorder. Again, it sometimes happens that bowel complaint takes place on very slight and trivial causes, and though disappearing for a time under the use, often, of remedies exceedingly trifling, repeatedly recurs till it becomes almost habitual, and wears down the patient by a slow but sure process of atrophy and debility ; and it is never imagined that this bowel complaint was only an effect of disease of the kidney, till perhaps after death, when it is ascertained by inspection that the structure of the kidneys is very much changed.

The large number of deaths referred to Mortification in both years, viz. 581 in the latter half of 1837, and 1343 in 1838, the majority in both years being males, would require more specific information. Mortification is the effect of rose, or of disease of the arteries; and it is curious that here the deaths ascribed to mortification are much more numerous than those ascribed to rose.

Atrophy we cannot help regarding as a very improper denomination for a cause of death. It is always the effect of disease in some internal organ, more or less essential to the performance of one or other of the important functions of the body. Thus it may be the result, and often is the result, of disease of the kidney, abscess of the kidney, abscess of the spleen or pancreas, chronic pleurisy, and various other internal processes of disorganization.

The number of deaths ascribed to this cause is, in the latter half of 1837, 478 males, and 481 females, 959 of both sexes; and in 1838, 1022 males, and 996 females, or 2018 of both sexes.

This is a large amount of persons whose deaths may be regarded as quite unascertained.

Sudden deaths are generally the result of disease of the heart or large vessels, especially by the way of hemorrhage. The number in the latter half of 1837, ascribed to this cause, was 634 males and 419 females, 1017 of both sexes ; and in 1838, 1840 males, and 1172 females, or 3012,—an increase of one-half, which is very great.

The last section in the list is formed upon a different, and perhaps a more complete model in the Report of 1838 than in that for 1837. It includes in the latter year, along with the list of violent deaths, deaths by intemperance, deaths by starvation, which were previously on the list of diseases of uncertain seat. The deaths ascribed to starvation in 1838 amounted to 167 ; in the latter half of 1837, they were 63, which is equivalent to 126 per annum. There is, therefore, an increase in this article, which, perhaps, was not to be expected, since the deaths from this cause are always more or less under the control of human agency.

It is certainly melancholy to reflect, that so many deaths should proceed from this cause, in a country where means so ample are provided to guard against the privations and destitution with which poverty is surrounded. But when it is remembered, as it ought always to be, that the injurious influence of the want of food is powerfully aided by the want of clothing, the want of shelter from the inclemency of the weather, and the want of all those little comforts which such destitution implies, it ceases to be wonderful that deaths should proceed from this cause ; but it may be singular, that it does not induce a greater number of deaths. The mode in which this operates, and also the kind of effects it produces, are so well explained by Mr Farr, that we think it right to give his account of these effects.

> " It will be seen that in the half year the deaths of 63 individuals were ascribed (principally at inquests) to starvation ; this is almost one annually to a population of 111,000. The want of food implies the want of everything else—except water—as firing, clothing, every convenience, every necessary of life, is abandoned at the imperious bidding of hunger. Hunger destroys a much higher proportion than is indicated by the registers in this and in every other country ; but its effects, like the effects of excess, are generally manifested indirectly, in the production of diseases of various kinds. The privation is rarely ever absolute ; the supply of food is inadequate to supply the wants of organization, which requires daily animal or vegetable matter containing not less than nine ounces of carbon."—P. 75.

On the same subject we find the following remarks in the Second Annual Report for 1838, and these are so important that we cannot withhold them from our readers.

"The 36 cases ascribed to starvation in 1837 have been since carefully investigated. 24 were infants that died for want of maternal nourishment; 12 died of inanition; 12 died from exposure to cold; 15 from the want of proper food, or from the want of the necessaries of life. The coroners were not the informants in the majority of instances. In 1838, the deaths of 126 males and 41 females, making 167 in all, were classed under Starvation.

"A detailed abstract was made of the deaths ascribed solely or partially to starvation by hunger or cold in 1838. The table comprised 123 cases, exclusive of infants under 1 year of age; and inquests had been held on the bodies in 77 cases. The following are examples of the various singular complications of cold and the want of food; in some instances the effect of accident, but more frequently of destitution.

"The cause of death as registered; the sex, age, profession, place and time of death, and the informant when an inquest was not held, are stated.

"1. Died in consequence of the inclement state of the weather and from want of the common necessaries of life; female; age, about 50; profession not stated; inquest; died at 9, Lincoln-court, Drury-lane; February 15th.

"4. Admitted in a destitute and dying condition; male; 65; pauper; informant, master of St George's Workhouse; died in St George's Workhouse; February 15th.

"7. Natural death, accelerated by destitution; male; 17; mendicant; inquest; died in the Holborn Union; February 20th.

"8. The effects of starvation and cold; female; 60; pauper; informant, master of workhouse; died in St Luke's Workhouse; January 28th.

"10. From exhaustion, induced by the extreme severity of the weather, acting on a body thinly clad, fatigued, and in want of nutritious food; female; 61; inquest; died at Clapham; February 15th.

"11. Want of proper and sufficient food; female; 29; single; inquest; died in St Mary's, Nottingham; February 11th.

"12. From destitution, caused by the inclemency of the weather; male; 76: baker; informant, master of workhouse; died at Westbury; February 5th.

"14. From the want of the common necessaries of life and the inclemency of the weather while in a boat off Trinity Sound; male; 13; sailor; inquest; Caistor Union; January 25th.

"15. Starvation; female; 43; pauper; informant, master of workhouse; Lincoln Workhouse; January 30th.

"16. Starvation; male; 44; shoemaker; informant, Lucy Hale; Stourbridge; January 22d.

"19. Cold, fatigue, and hunger; male; 47; grocer; inquest; Haslingden; March 9th.

"25. From want of the common necessaries of life; male; 50; profession unknown; inquest; Charing Cross Hospital; June 15th.

"26. Destitution; male; 42; labourer; informant, William Wix; St Bartholomew's Hospital; April 1st.

"29. Ague, accelerated by destitution and want; male; 45; vagrant; inquest; Hawkhurst; April 1st.

"30. Destitution; female; 1¾; daughter of W. and H. Cleaver; informant, Ann Young; Oving; May 7th.

"31. Inanition; male; 70: profession unknown; informant, master of workhouse; St Giles's Workhouse; July 31st.

"40. Starvation,—viz. the want of sufficient nourishment during illness; male; 55; labourer; informant, C. Patriot; Baptist Street, Carnarvon; November 26th.

"43. Hunger and cold; male; 34; inquest; Thame; December 10th.

"48. Starvation, but no evidence appears to the jury of any application having been made to the Board of Guardians or the relieving officer for the last two months; male; 18; vagrant; inquest; Ripley; November 17th.

"50. Died from starvation; male; 58; gentleman; inquest; Headington; April 7th.

"53. Starvation; male; 43; tailor; inquest; Kentmere; October 29th.

"54. Starvation, (father of No. 55); male; 50; labourer; inquest; Kentmere; October 29th.

"55. Starvation, (son of No. 54); male; 14; inquest; Kentmere; October 29th.

"61. Died from cold and exposure to the weather,—verdict of inquest; male; 25; vagrant; informant, W. Seaman, constable; died at Cheam; January 14th.

"63. Perished in the snow from the severity of the weather; male; 18; labourer; informant, master of the workhouse; died at Chatham; January 18th.

"67. Frozen to death; male; 60; vagrant; informant, G. Burl, undertaker; died at Leominster; January 20th.

"70. Fatigue and intense cold; male; 54; land-surveyor; inquest; died at Holt; January 20th.

"86. Starved to death by cold; male; 60; pauper; informant, J. Jacob, House of Industry; died at Shrewsbury; January 19th.

"99. Died in a state of starvation from cold; male; 18; mariner; informant, E. Rodriguez; died at Liverpool; May 1st.

"123. Starvation from neglect of mother; male; 21 months; son of a spinster; informant, Martha Brook; died at Huddersfield; September 3d."—P. 7.

This is a fearful list of instances in which the thread of human life has been abruptly and prematurely cut short, or rather snapt asunder by the want of what are usually called the common necessaries of life. Nothing can demonstrate so forcibly the necessity of ascertaining with precision the causes of death as this list; and, if all the facts be established, nothing can establish more decidedly the necessity of some system of precautionary inspection and temporary relief, by which accidents of this description might be prevented.

It must, nevertheless, be observed, that it is rarely from pure want of food alone that death is produced; and this privation, which it has been well observed by Mr Farr is seldom complete or absolute, requires to have its pernicious influence aided by the concurrent operation of various auxiliary causes, for instance, long exposure to cold and inadequate clothing. With regard to the term "want of the common necessaries of life," we believe it will be found that this is a very rare circumstance. It is more likely that it is the want of several little comforts that operates in accelerating the approach of the king of terrors, than the absolute want of necessaries. Among these may be enumerated want of adequate clothing, want of fuel, want of comfortable beds and bed-clothing, want or neglect of the means of personal cleanliness, all of which operate in enfeebling and perverting the functions, and thereby rendering the sufferers less capable of resisting the direct effects either of cold, or of scanty and precarious supplies of food.

The announcement of so large an amount of cases of death by starvation has created a good deal of discussion and observation upon the operation and application of the New Poor Law; and it has been very generally believed, that these casualties are in a great degree, if not altogether, to be ascribed to the mode in which relief is afforded under the provisions of the Act, that alleged scanty and stinted manner in which it is afforded, the difficulties thrown in the way of paupers obtaining relief, and, above all, the refusal of all out-door assistance. It would lead us far beyond the limits within which these observations must be restricted, were we to enter into even a very general consideration of the grounds of these charges against the New Poor Law. But it seems to be totally overlooked that no poor law, however perfect in principle, and however administered, can provide an effectual remedy in all cases against destitution and its effects, in such circumstances as those specified in the report. Poverty, indigence, and destitution are necessary evils in almost all states of human society, both savage and civilized; only differing in the modes and forms which they assume; and to suppose a state of society without these adjuncts, is to suppose a state of society not consisting of human beings, with all their faults of improvidence and misconduct, and all those which are the consequences of ignorance, misfortune, and sometimes of vice and guilt. All that can be done is to diminish the number and frequency of accidents of this description; to diminish as far as may be possible the amount of human misery and suffering; and to alleviate those necessary consequences of them which cannot be wholly removed.*

---

* Since these observations have been written, we observe a most melancholy case of death by inanition, in a powerful, large, labouring man, recorded in a letter in the Times of Friday 11th; and if all the facts be correct, it is scarcely possible to doubt, that the guardians behaved to this poor man with unjust and unchristian harshness.

It is important here to observe, that the amount of deaths by starvation is much greater in large and populous cities and their neighbourhoods than in moderate-sized or small towns, and in country districts. Thus the mortality from this cause in 1838 was in the metropolis 29; and in the unions of the five counties of Cornwall, Devonshire, Dorsetshire, Somersetshire, and Wiltshire only 14, not so much as one-half, took place, (Table D,) p. 50. From another tabular view, on the other hand, of the diseases of town and rural districts, in which the diseases of various towns, containing an aggregate population of 1,484,402 persons, are contrasted with those of twenty-four rural districts, containing an aggregate population of 1,656,455, the number of deaths by starvation was fifteen in the towns and seventeen in the counties; which is very nearly at the same rate, compared with the numbers of the population.

These facts would lead to the inference, that deaths by starvation were about twice more frequent in the metropolis than in rural situations, and that they were very nearly of the same degree of frequency in moderate-sized towns as in other country places. It would be rash, however, to draw from this any positive general conclusions, more especially as it appears that in some counties, not remarkable for large or populous towns, the number of deaths by starvation is considerable. Thus in a district embracing the northern parts of Lincolnshire, Rutlandshire, Derbyshire, Nottinghamshire, Leicestershire, and Northamptonshire, with a population of about 1,400,000, twenty-eight deaths are ascribed to this cause; in the counties of Oxford, Gloucester, Worcester, and Warwick, excluding the towns of Dudley and Birmingham, with a population of more than 1,000,000, not fewer than thirty-six deaths are ascribed to starvation; in Cheshire, Shropshire, and Staffordshire, excluding the mining districts, with a population of 800,000, eighteen deaths are imputed to this cause; in Wales, Monmouthshire, and Herefordshire, with a population of 1,100,000, eighteen deaths took place from this cause; and in Lancashire, excluding Liverpool and Manchester, and in Cumberland and Westmoreland, sixteen deaths took place in each, or thirty-two in the two districts.

Upon the subject of the diseases reputed epidemic, endemic, and contagious, we have already anticipated, by the foregoing remarks, many of those which these distempers had suggested, and we have little to add to what has been already said. We would, indeed, expunge from this list, certainly, croup, thrush, diarrhœa, dysentery, cholera, and probably erysipelas, which, though occasionally seen to present the characters of an epidemic, and though by several respectable physicians believed to be sometimes propagated by contagion, forms, nevertheless, so small a proportion of sickliness that it might, perhaps, as well be ranked with cutaneous or gastro-cutaneous disorders. On this, however, we will not dwell; and we think there is less chance of fallacy in leaving rose among the epidemic distempers than others which are placed there.

With the two last diseases of the list, viz. syphilis and canine madness, we think it is a little different. Though syphilis is unquestionably a contagious disease, yet it presents neither epidemic nor endemic characters. It is the effect of an animal poison; but this poison is neither applied, nor operates in consequence of epidemic or endemic circumstances; but in consequence of causes over which the parties have complete control. To place such a disease in this class conveys no useful information; and it would be better situate among disorders of uncertain seat.

Hydrophobia or rabies is, according to our ideas, not less improperly placed here. Though rabies in dogs may be at certain seasons epidemic, and though the disease be always propagated by contagion or rather inoculation, yet its prevalence in man does not depend so much on epidemic causes as to entitle it to a place in this part of the classification. Though the rabid virus applied by a bite does produce a specific disease, yet this disease cannot be said to be on the same footing with small-pox, fever, or scarlet fever; and, as no very useful information is communicated as to the prevalence of epidemic or contagious diseases, we think it might as well have been placed among the list of violent deaths, as death by the bite of a ferret, death by a lion, death by a viper, and death by the bite of a dog not hydrophobic.

The number of children destroyed by drinking boiling water from the spout of a tea-kettle is great in both years; in the latter half of 1837, 6, equivalent to 12 annually, and in 1838, no fewer than 25, which is double the amount of 1837. We doubt whether the fire-guard could prevent this accident. It is chiefly the result of the habit of using the tea-kettle both as a reservoir for cold water and a drinking vessel, and also as a vessel for boiling water; all from the want of appropriate drinking vessels, and the habit, consequently, of drinking from the tea-kettle. The children seeing the parents quench thirst in this manner, think there can be no harm in imitating them, and hence the number of fatal accidents from this cause.

From this subject we must pass to a few others introduced in the Reports; and we shall shortly advert to some points illustrating the statistical etiology of diseases.

Among the diseases of the organs of respiration pneumonia or pulmonic inflammation and consumption were by far the most fatal. The former appears to be most fatal to children. Among 379 fatal cases of pneumonia in the metropolis and in some country districts, 228 were children under three years of age. It is added that several of these cases were the sequelæ of measles and hooping-cough. Pneumonia seems to be fatal in children, chiefly from its being overlooked until it has proceeded too far to be amenable to treatment, and from the rapidity with which it advances in children.

The deaths from consumption amounted in the latter half of 1837 to 27,754, which is equivalent to 20 per cent. of the total number, or nearly four annually among 1000 living. In 1838, the mortality from consumption was 27,935 among males, and 31,090 among females, making a total mortality of 59,025, which is equivalent to two-thirds of the mortality from all the diseases of the respiratory organs, and between a fifth and a sixth part of the total mortality from all diseases. Consumption, Mr Farr observes, is 8 per cent. more fatal to females than to males. This he explains in the following manner:

"The higher mortality of English women by consumption may be ascribed partly to the in-door life which they lead, and partly to the compression, preventing the expansion of the chest, by costume. In both ways they are deprived of free draughts of vital air, and the altered blood deposits tuberculous matter with a fatal unnatural facility. *Thirty-one thousand and ninety* English women died in one year of the incurable malady! Will not this impressive fact induce persons of rank and influence to set their countrywomen right in the article of dress, and lead them to abandon a practice which disfigures the body, strangles the chest, produces nervous or other disorders, and has an unquestionable tendency to implant an incurable hectic malady in the frame? Girls have no more need of artificial bones and bandages than boys."—P. 5.

It appears to us very doubtful whether this cause be sufficient to account for the preponderance of mortality by consumption among females more than males. Of the effects of within-door life it is difficult to speak positively; for in the lower ranks many females seem to be attacked by consumption while not leading lives properly within doors; and the alleged injurious influence of the mode of life seems to be much diminished, if not annihilated, by the two facts,—that while all females dress much in the same manner, only a certain proportion are attacked by and fall victims to the disorder; and that males, who use no articles of dress which can injure the chest, and who ought therefore to be exempt, are subject to the disease to an amount not much smaller than females.

The following table, exhibiting the ages of 1200 persons destroyed by cancer, is important:

Years.	Und. 5	5—0	10—15	15—20	20—30	30—40	40—50	50—60	60—70	70—80	80—90	90	All Ages.
Males, . . .	3	3	1	3	5	21	42	69	89	65	17	3	321
Females, .	9	2	4	5	15	66	175	250	198	118	36	1	879
Males and females,	12	5	5	8	20	87	217	319	287	183	53	4	1200
Mean Annual Mortality in 100,000 living at each Age.	1.2	.6	.6	1.3	1.7	10.5	56.1	67.8	91.9	117.8	108.9	17.4	...

This shows at once how much more frequent cancer is among females than among males. Between 20 and 40, the number of females attacked is three times greater than of males; between 40 and 60, the number is four times greater; and between 60 and 90 it is more than twice. The disease is most frequent between the ages of 30 and 70. The occurrence of cancer below the age of 15 may be well founded; but is there not reason to believe that in some young subjects the disease is rather of the encephalomatous character? Genuine fibro-cartilaginous carcinoma is, we believe, a rare disease before the age of 40; and it seems singular that so many as twelve cases should take place in the twelve before the age of 5 years, while at five years only five cases take place, and between 10 and 15 years the same number. All the cases said to take place under the age of 20 are most likely to be ascribed to soft cancer or encephaloma.

Suicide is arranged under the head of violent deaths. In 1838, the number of suicides is stated to be 1058, of whom 751 were males and 307 females. On the different modes adopted to accomplish self-murder, no information is given; and we beg to suggest, if it be practicable, that this might furnish a curious, if not useful department of statistical information. The ages are distinguished, the counties in which they took place, and the season of the year at which they were committed, in the following table:

The Ages of 1044 suicides.

	10—15	15—20	20—30	30—40	40—50	50—60	60—70	70—80	80—90	90
Males and females,	8	73	151	172	217	212	141	54	14	2
Suicides to 100,000 living at each age,	0.4	4.7	6.2	9.3	14.9	20.6	19.8	15.2	14.3	...
Series increasing 49.6 per cent.	...	...	6.2	9.2	13.8	20.7	...	...	...	...

The Tendency to Suicide is greatest in the Metropolis, and is least in Wales.

	Metropolis.	S.E. Count.	S. Midland Counties.	E. Counties.	S.W. Count.	W. Counties.	N. Midland Counties.	N. W. do.	Yorkshire.	N. Counties.	Wales and Monmouth.	England and Wales.
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	
Males,	178	95	45	53	71	51	49	90	67	37	15	751
Females,	59	41	20	17	25	33	27	35	21	19	10	307
Males and Females,	237	136	65	70	96	84	76	125	88	56	25	1058
Suicides to 100,000 inhabitants,	12.6	9.3	5.8	6.5	5.5	4.5	6.9	6.3	5.7	7.2	2.5	6.8

The smallest Number of Suicides occurs in the Cold Season of the Year.

	M.	F.	M. and F.
January, February, March, .	163	66	229
April, May, June, . . .	210	94	304
July, August, September, ·	214	73	287
October, November, December, .	164	74	238

The greatest number of self-murders appears to be committed by persons between the 40th and 60th years of age; and the largest proportion among those actually living between 50 and 70 years. It appears that between the ages of 50 and 60 twenty per 100,000 commit self-murder; and between those of 60 and 70 the number per 100,000 is 19.8, very nearly twenty.

As might be anticipated, the tendency to self-murder is greatest in the metropolis, least in Wales and the conterminous counties, and at a middle rate in the south-eastern and the northern counties. It is not difficult to understand the reason of the frequency of this accident in the metropolis. The mental powers and emotions are there kept in a state of almost constant fervent commotion, or what is named violent excitement. All those objects which rouse men to great and excessive exertions, intellectual and corporeal, are incessantly operating upon the mind, at the same time; the chances of great success or of total ruin, of boundless wealth or extreme poverty, the most brilliant prosperity or the most calamitous adversity, are there all so closely connected, that the mind is exactly placed in the position calculated to create the most sanguine expectations, or to engender despair and extinguish hope, especially in persons whose passions, either of avarice or of ambition, tempt them to stake largely either in commerce or in similar pursuits.

The comparison of the diseases of towns and rural districts gives rise to a very important and useful inquiry. It has long been taught in general terms, that towns are most unhealthy; that large and populous cities are the graves of the human race, and could not maintain their own population without the incessant supplies which are furnished by the inhabitants of the rural districts, flocking thither with the hope of making fortunes; and that the country is the only situation, in which the human race can preserve health and attain a moderate degree of longevity. But though these propositions have been stated in these general terms, no direct proof of their accuracy has ever been adduced; and the subject has been treated with little precision. The following observations by Mr Farr are deduced with care, from the comparative examination of the diseases in both situations on a large and extended scale:

Deaths by twelve classes of fatal Diseases in City and in County districts.

	Cities.	Counties.
Population, . . .	3,553,161	3,500,750
Epidemic, endemic, and contagious diseases,	12,766	6,045

Sporadic Diseases.	Cities.	Counties.
Of the nervous system,	7,705	3,607
Of the respiratory organs,	12,619	7,847
„ organs of circulation,	590	309
„ digestive organs,	3,476	1,832
„ urinary organs,	219	161
„ organs of generation,	460	265
„ organs of locomotion,	262	154
„ integumentary system,	62	55
Of uncertain seat,	4,396	3,730
Age,	2,924	3,102
Violent deaths,	1,370	929
Not specified,	1,104	1,657
Total,	47,953	29,693

" The concentration of the population in cities doubles the deaths from the two first classes of disease ; the ratio of deaths having been as 1 to 2.11, and 1 to 2.13 ; and upon reference to the individual diseases in tables C., D., it will be observed that the augmentation in the latter class occurs principally in convulsions and hydrocephalus :—Deaths by convulsions, counties, 1,347, cities, 3,723, ratio 1 : 2.76 ; by hydrocephalus, counties, 559, cities, 1,540, ratio 1 : 2.75. It has already been intimated, that convulsion is a frequent intercurrent symptom in diarrhœa and diseases of the epidemic class in infants ; it may exist, however, as an independent affection, and in that case has clearly, as well as hydrocephalus, with which it is allied, an epidemic character. A similar remark will apply to pneumonia and bronchitis, of which 1,209 cases were registered in the counties, 2,865 in the cities ; ratio 1 : 2.37. The pulmonary inflammation was, in many cases, developed in the course of measles, influenza, and other diseases of the first class. The three following diseases, which principally affect adults between the ages of 15 and 65, show that unhealthy places augment the fatality of diseases in different degrees.

	Counties.	Cities.	Increase per cent. in Cities.
Deaths by consumption,	5,857	8,125	39
" child-birth,	217	372	71
„ typhus,	1.564	3,456	221

" This gives the classification a peculiar property. Wherever the absolute mortality is low, the number of deaths in the epidemic class is less than the number in the pulmonary class ; and, on the contrary, wherever the deaths in the first class exceed or equal those in the third, it may be affirmed that the absolute mortality is high."—Pp. 77, 78.

A view of the deaths in the city districts and the country districts for 1838, leads to similar results. The following observations, with the accompanying table, present these results in a short but intelligible form :

Deaths by twelve Cases of Fatal Diseases in City and in Rural Districts.

			City Districts.	Rural do.
		Estimated population, June 30, 1838,	3,726,221	3,539,908
1.		Epidemic, endemic, and contagious diseases,	23,655	13,685
2.	Sporadic Diseases	Of the nervous system, - - -	15,651	8,177
3.		Of the respiratory organs, - - -	28,973	18,508
4.		Of the organs of circulation, - -	1,301	712
5.		Of the digestive organs, - - -	6,505	3,361
6.		Of the urinary organs, - - -	417	373
7.		Of the organs of generation, - -	984	547
8.		Of the organs of locomotion, - -	653	354
9.		Of the integumentary system, - -	144	66
10.		Of uncertain seat, - - - -	10,447	10,529
11.		Age, - - - - - - - -	7,374	8,874
12.		Violent deaths, - - - - - - -	3,104	2,516
		Causes not specified, - - - - -	1,811	2,708
		Total deaths, - -	101,019	70,410

" Besides the 70,410 persons who died equally in the dense and in the more scattered populations, there was an excess in the cities of 30,609 deaths; 9970 from diseases of the epidemic class, 7474 from diseases of the nervous system, 10,465 from diseases of the respiratory organs, and 3144 from diseases of the digestive organs. The annual rate of mortality in the cities was 2.7, in the counties 2.0 per cent.; and the mortality in the cities 1.36 to 1.00 in the counties. The mean duration of life in the two sets of circumstances would differ nearly in the ratio of 37 years and 50 years.

" In examining the special causes of death, three classes may be distinguished; one class which was exaggerated in cities to the highest pitch, a third class in which the mortality was nearly the same or in excess in the counties, and an intermediate class. To 1.00 deaths in the counties the deaths out of the same amount of population in the cities were by asthma, 3.80; erysipelas, 2.71; convulsions and teething, 2.57; cephalitis and hydrocephalus, 2.41; hydrophobia, 2.37; pneumonia, bronchitis, and pleurisy, 1.99; delirium tremens, 1.98; typhus, 1.88; small-pox, 1.73; heart-disease, 1.73; childbirth, 1.63; syphilis, 1.59; rheumatism, 1.58; gout, 1.55; hernia, 1.48; purpura, 1.46; sudden deaths, 1.45; liver disease, 1.45; hepatitis, 1.35; tetanus, 1.32. The excess of mortality in cities was less in the following cases: by consumption, 1.24; croup, 1.23; violent deaths, 1.17; stone, 1.11; mortification, 1.10; malformations, 1.07; apoplexy, 1.07; hæmorrhage, 1.02.—The mortality by the third class of causes was greater in the counties than in the cities: for the mortality to 1.00 in the counties was, in the cities, by paralysis, .99; dropsy, .99; jaundice, .99; diabetes .97; cancer, .92; hydrothorax, .88; hematemesis, .79; debility (frequently premature birth,) .75; atrophy, .75; scrofula, .46. It will be useful to compare all the other diseases in Table (F); but, in doing so, it must be borne in mind that the diseases in the epidemic class fluctuate from year to year; that when the number of cases is considerable, the relative mortality is most correctly expressed, and that slight differences deserve little attention.

"The fatality of scrofula, purpura, cancer, stone, jaundice, diabetes—chronic diseases—in which there are new deposits, new formations, or new secretions, is as great in the country as in cities; dropsy comes under the same head. Their exciting causes are common to the two classes of population.

"It has been proved that the same injuries and diseases are more deadly in cities than in the country; which may account for the higher mortality from violent death, hernia, and some other causes. Parturition is as frequent in the country as in town; where it is nevertheless so often followed by puerperal fever as to be 63 per cent. more fatal."—Pp. 9, 10.

Mr Farr allows that the manner in which human beings are aggregated, and as it were accumulated, in cities and towns, is extremely favourable to the developement and the propagation of many fatal disorders, and that the same diseases are thus rendered greatly more severe and unmanageable, and more fatal in cities and towns than in the open country, where the air is more pure, and where ventilation is more perfect. But he is disposed, on the other hand, to doubt whether the increased mortality of cities be an evil necessary to their construction, or only adventitious,—be an inevitable result of the modes of life there pursued, or one which can in various modes be counteracted; and he further directs attention to the fact, that in country situations, the labouring classes are exposed to hard labour, bad or imperfect accommodation, coarse scanty fare, and are thus often attacked by disorders, which persons living in towns either wholly escape, or have in small numbers and in mild form. This he thinks is especially true of epidemic disorders, where the affluent are exposed to the same general causes of sickliness, but where, from possessing the means of more thoroughly protecting themselves against their operation, they suffer in a less degree, and often, by enjoying such means of shelter, clothing, food, and cordials, as counteract the injurious operation of morbific agents, either present these disorders in a mild form, or altogether escape them. Mr Farr refers in proof of the justice of this view to the fact, that what may be styled "the health of London," or more accurately, the salubrity of the city, has been greatly improved during the last two centuries, and that in certain districts the sickness and mortality are much smaller than in others.

We find some judicious observations on the influence of climate and on that of seasons; but, the limits, within which these remarks must be confined, prevent us from entering into them at length. On the latter subject, one or two remarks deserve to be made. In pestilential years, as in 1603, 1625, 1636, and 1665, when the annual mortality was 25 per cent. or one-fourth of those living, the largest mortality took place in the months of July, August, and September, (16.2.) In intercurrent years, or those in which there was no plague, and when the annual mortality was 7 per cent., the mortality was greatest in the same months, (2.1), but only a single tenth more than in the months of October, November, and December, (2.) In 1838, on the contrary, when the mortality is only 2.81, the whole of this is reversed. The months of July, August, and September are remarkable for being those in which the mortality is smallest, (6.0); the mortality is increased in October, November, and December, (6.6); it is still higher in the spring months of April, May, and June; and it is highest in the months of January, February, and March. This change chiefly depends on the great change which has taken place in the prevalent fatal diseases. In the first period, that is, the latter half of the seventeenth century, the mortality was mostly caused by such disorders as plague, fever, dysentery, and ague. In 1838, though much mortality is still caused by fever, yet plague is not known; dysentery is seldom fatal as an epidemic; ague is rarely fatal by itself; and the great mortality is caused by consumption, pneumonia, small-pox, and similar diseases. In the metropolis in 1838, excluding deaths from old age, by far the greatest mortality was caused by bronchial, or pulmonary, or pleuritic inflammation; and the deaths from these diseases were more numerous in winter and autumn. Next to these, asthma destroys the greatest number of persons during the winter season; then comes dropsy, that is, disease of the kidney we infer, and disease of the heart, during the winter, and the same malady in autumn and in spring; and after this the numbers present nothing very remarkable. The mortality by consumption, though very high in winter, (1944), is, however, still greater in spring, (2109). In summer it is nearly the same as in winter; and in autumn it is lowest. But it is not remarkable that a disease, which is slow in progress, chronic in character, and complicated in nature, should in this manner destroy most of its victims at a season, when the length and severity, as well as the variableness of the winter may have been slowly consuming the texture of the lungs, wasting the flesh, and reducing the patient to that wretched state of debility in which the smallest changes in atmospheric temperature must produce very great changes in the human frame.

It is remarkable that in this table, (p. 15) the number of males destroyed by consumption in the metropolis is greater than that of females. The former amounts to 4057; the latter to 3630, throughout the year. This disease, therefore, is in the metropolis more fatal to males than to females.

This letter of Mr Farr is concluded by an account of an epidemic of small-pox which appears to have prevailed in many parts of England and Wales, from the 1st of July 1837, to the 31st of December 1839; at least, a space comprehending two winters, two springs, three summers, and three autumns. This large epidemic consisted of several small epidemics, prevailing as is usual at different places during the period of its visitation. Its course is described by Mr Farr in the following manner.

"When the Registration Act came into operation, the epidemic of small-pox had commenced, and was rapidly advancing. It was raging at its height on the western side of the island. In Liverpool and West Derby 458 individuals perished, and were registered under small-pox in the first three months. Bath, with a much smaller population, lost 154 lives; Leicester, 43; Shrewsbury, 35. The epidemic prevailed in the south-west counties in autumn, extending to the districts around Bath, and then passing from Somersetshire to Devonshire, where it destroyed 131 lives in Exeter, and half as many more in the surrounding districts; and to Wiltshire, where 40 died in Calne, Marlborough, and Pewsey, 48 in Devizes, and 22 in Salisbury. It penetrated further into the country: 64 died in Wycombe, 72 in Wolverhampton, 57 in Blackburn, 99 in Wigan. The deaths from small-pox in Wales were tripled: 69 died in Wrexham, 85 in Abergavenny and Pontypool, 54 in Merthyr Tydfil. The hills and the valleys of Wales were traversed, and 711 victims were cut off, in the third quarter, the winter of 1838. The disease hovered over the Metropolis at the first: 22 died in Holborn, 10 in Whitechapel, 16 in St George's, Southwark, 29 in Lambeth, 47 in Greenwich: the deaths from it were doubled in the second quarter; 753 perished in the winter of 1838. The surrounding districts were infected, Richmond, Kingston, Brentford, Staines, and Uxbridge; Dorsetshire, in the south-west, that had remained almost exempt, was visited; Weymouth, Bridport, Beaminster; then Sherborne, Dorchester, and Cerne; and Taunton that had been just left by dysentery, with Williton, Wellington, and Bridgwater in Somersetshire. During the winter quarter (1838) not less than 121 died in Bristol and Clifton; 63 in Worcester, 36 in Dudley, 61 in Wolverhampton, 108 in Birmingham and Aston, 40 in Altrincham and Runcorn, 156 in Manchester and Salford. The small-pox mortality attained its maximum in the spring of 1838: the metropolis saw 1145 carried to premature graves; Surrey lost 83 by the malady; Kent, 132; Berkshire, 64; Wiltshire. 93; Somersetshire, 222; Gloucestershire, 142; Worcestershire, 89; Warwickshire, 107; Lancashire, 442; Yorkshire, 282; Durham, 88; Cumberland, 44; Monmouthshire, 82; Wales, 515. In three months, 4489 deaths from small-pox were registered. The epidemic paused, either because its strength was exhausted or its victims failed; yet 3685 fell under its hands in summer, 3851 in autumn. On the Surrey side of the Thames, and at the west end of the metropolis, the mortality attained the highest pitch in the summer and autumn of 1838; in the three last months of the year, 104 died of small-pox in the Westminster district. Ramsgate and Margate suffered severely. In the summer 76 died at Reading; in autumn, 62 in Ely, North Witchford, and Wisbeach, Cambridgeshire; 50 in Romford, Orsett, Billericay; 48 in Rochford and Maldon; 78 in Colchester, Essex; 95 in Ipswich; 48 in Plymouth; 180 in Manchester; 106 in Oldham; 197 in Leeds; 22 in Whitehaven; 38 in Westmoreland.

"In 1839 the epidemic reached Norfolk: in the spring 127 were destroyed in Walsingham, Docking, Freebridge Lynn, and King's Lynn; 180 in Norwich, where 204 more died in the ensuing summer; the disease still raging in Essex and Suffolk, but with diminished violence. The North and the East Ridings of Yorkshire, Sunderland, Newcastle-upon-Tyne, and Tynemouth, were visited. The epidemic subsided on the eastern shore; and in the summer of 1839 only 1533 died of small-pox in the kingdom: 65 cases of small-pox were registered in London. In the autumn of 1839 signs of a second epidemic appeared at Liverpool, Bath, and other towns; the deaths in the kingdom rose to 1730.

"The epidemic destroyed more than 30,819 persons."—p. 17.

It appears that, in the metropolis for a long time past, five deaths take place every week in consequence of small-pox. This is when the disease is sporadic, and does not prevail in its epidemic character. When the mortality rises much above five deaths in the week, Mr Farr gives us to understand that he considers the disease as dependent on epidemic constitution. Under such circumstances, the mortality rises to 10, 15, 20, 30, 40, and 70 per week. In the year 1838 the average weekly mortality by small-pox was 73 persons. It appears from the weekly tables with which we have been favoured from the Registrar's Office for the current year, that the mortality from small-pox varied during the months of January, February, March, and April, from 4 to 12 per week. In May they rose to 15; and fell again in June to 10. In July and the first weeks of August, they varied from 10 or 11 to 13; but in the end of that month rose to 25, and have since that time varied from 20 to 35. In November and the present month the weekly deaths by small-pox rose from 43 to 60, mostly in persons under 15 years of age. According to this list, therefore, small-pox has been epidemic in London for the last six months.

There is no doubt, that, while vaccination acts as a protecting agent against the attacks, and diminishes much the mortality of small-pox, inoculation, by which the variolous poison is diffused through the community, operates as an exciting cause of the disease, and thereby tends to increase the mortality from this source. A very great actual benefit will be secured to the public by the clause in the New Poor Law Act, which forbids the practice of inoculation, and enforces vaccination among the parochial authorities, under the infliction of severe penalties.

Various other important subjects in these Reports deserve consideration. But our limits prevent us from entering further into them at present; and we must defer to a future opportunity the notice of several important circumstances in the etiology and prevalence of disease, on which they furnish valuable information. Our chief object in directing attention to them at present is to induce our readers to study these Reports, and thereby to be enabled to rectify several prevalent errors on the alleged frequency of certain disorders. We are also anxious to see the nosological arrangement somewhat altered, and if possible improved, by being rendered more precise and accurate. On this point, however, we speak with that deference to the judgment of Mr Farr to which his attention to this subject, and his knowledge of it give him a just title. We know that he will take in good part the observations which we have seen occasion to make, and the suggestions which we have found it incumbent to offer. Their adoption we do not expect, unless they are found at once necessary, practicable, and expedient. But, under all circumstances, we are satisfied that the compilation of these Reports with all their defects, shows not only indefatigable industry and great talent, but will also be highly useful in illustrating the etiology of disease, and the nature of those distempers which are most fatal to the human race.

ART. IV.—*Transactions of the Medical and Physical Society of Bombay.* Volume i. Bombay: Printed at the American Mission Press, 1838. 8vo. Pp. 370.

1. *A Report on a Disease which prevailed in part of Kutch and Kattywar in the years* 1815 *and* 1816. Addressed to the Secretary of the Medical Board. By J. M'ADAM, Esq. Superintending Surgeon, Deccan Division, formerly Civil Surgeon in Kattywar.
2. *A Report on a Disease which prevailed in part of the Zillah of Ahmedabad in the years* 1817, 1818, *and* 1819. Addressed to the Secretary of the Medical Board. By J. GILDER, Esq., formerly of the Bombay Service.
3. *A Report on a Disease which prevailed in Kattywar and in parts of the Zillah of Ahmedabad, in the years* 1819 *and* 1820. Addressed to the Secretary of the Medical Board. By THOMAS WHYTE, Esq., formerly of the Bombay Service.

IN our fifty-first volume we gave an account of a pestilential fever which had been raging in 1836 and 1837, with great mortality among the natives of Pali and the western provinces of India; and, at the same time, stated our purpose of directing attention, as soon as possible, to the accounts of a similar epidemic which prevailed in Kutch and Kattywar, in the years 1815 and 1816, and in the Zillah of Ahmedabad in 1819 and 1820. We now proceed to fulfil the promise then made. But it is requisite previously to advert shortly to the origin of the publication, to which we have been indebted for information regarding this disease.

In November 1835, a number of medical gentlemen, attached to the different divisions of the public service in the Presidency of Bombay, impressed with the conviction of the benefits which had resulted from the scientific labours of the Medical and Physical Society of Calcutta, in collecting, digesting, and publishing a great deal of useful information on the medical topography of India, and the diseases peculiar to its particular regions, and to different Indian tribes, and conscious that they enjoyed similar opportunities of procuring information on many subjects which might be useful to themselves, and highly instructive to those who were daily coming to India, determined on instituting a similar association at Bombay, for the purpose of promoting the advancement of medical science and its collateral branches. On the 23d of that month, accordingly, there was held, at the office of the medical storekeeper, a meeting of all the leading medical officers then in the city, at which, with Mr Collier, Deputy-Inspector General in the chair, and Dr Morehead, as secretary, a series of resolutions was read, expressive of the general sentiments of the meeting, and establishing an association much on the same principle as that of the Medical and Physical Society of Calcutta.

The society thus constituted was rapidly enlarged by the frequent accession of new members; and before the close of 1838, the society consisted of eighty-eight ordinary resident members, four non-resident ordinary members, and six corresponding members.

One peculiar feature in the society arose out of the circumstances of the members, few of whom being resident in Bombay, could conveniently attend regular periodical meetings. For the present, therefore, these were deemed unnecessary. Papers, however, and dissertations, intended for the society, were sent to the secretary for perusal; and to him was assigned the duty, after preparing an abstract for publication, of circulating them among the members resident in Bombay and Poonah, and subsequently placing them among the records of the society. A great number of papers was thus in a short time collected; and from these the committee of management selected such as it seemed expedient to publish.

In this manner the materials of the present volume have been collected and published. In noticing the appearance of this first effort of the society, it is right to say that great merit is due, not only to the committee, but also to Dr Charles Morehead, both in his capacity of member of that committee, and also in the performance of his duty as secretary. These duties have not been light or easy of performance; and his zeal and interest in the cause, with his indefatigable industry, have enabled him to carry into effect a measure which nothing but great zeal and industry, as well as much influence, could enable him to accomplish. It is gratifying to us to observe and to be enabled to say, that Dr Morehead, who must be known to many in this city, as the son of the well-known and much respected Episcopal clergyman, formerly of Edinburgh, and from being educated at this University, has been highly instrumental in providing a channel for preserving and making public many papers and documents illustrative of Indian diseases and accidents, which must have otherwise been forgotten or left in undeserved obscurity.

The contents of the volume are varied as well as interesting. Two papers on the Medical Topography, the one of the province of Guzerat, the other on the Climate of the Mahabuleskwur hills, contain much useful matter, and may afterwards become the subject of attention. At present we propose to direct the attention of our readers to three papers, the titles of which are prefixed, on a fever with affection of the glands, which prevailed in various parts of the upper and western provinces of India, between 1815 and 1820, and which bore a close resemblance to that of which, under the name of the Pali Plague, we gave an account in our fifty-first volume.

The papers devoted to the history of the appearance, progress, and symptoms of this disease are three in number; and we shall consider them in chronological order, as the best calculated for giving a clear view of the progressive advancement and diffusion of the epidemic; at the same time taking from each such facts and observations as may give a consecutive account of the disorder.

The first paper on the subject is the Report by Mr M'Adam; the second paper is by Mr J. Gilder; and the third is by Mr Thomas Whyte.

One of the most remarkable and most constant circumstances in the history of all diseases reputed pestilential is, that their origin is involved in obscurity so perfect, that it is quite impossible to obtain any information on the first commencement of the distemper. This, Mr M'Adam informs us, was the case with the distemper of Kutch and Kattywar. It was first noticed in May 1815, at Kuntakote and its neighbourhood, a considerable town in that province; and thence spread rapidly to Munsurra, Chitore, Adooee, and Wandia, all towns in Wagur, in all of which towns it caused much mortality during January, February, and March 1816. In May 1816 it appeared in Moorvee, on the Kattywar side of the Runn of Kutch, and continued there to prevail with great mortality so late as November 1816. In August 1816 it appeared again on the opposite side of the Runn, and after prevailing there in various towns, spread into the province of Sinde, where, at the end of 1816, it continued to cause great mortality.

The following account of its appearance and characteristic symptoms are given by Mr M'Adam.

" The origin of the disease which has for some time raged in Wagur and the Mucha Kanta, is involved in obscurity. It appears to have been first noticed at Kuntakote or its neighbourhood, in May 1815. Thence it spread to Munsurra, Chitore, Adooee, and Wandia, all towns in Wagur; and in all of which it committed great ravages during the months of January, February, and March of the present year. In May last, after it had ceased to prevail in Wagur, it made its appearance in the town of Moorvee, on the Kattywar side of the Run; and there it has continued up to the present time to prevail with fatal effects. In August the disease repassed the Run to Buchau, twenty-three miles distant from this place, and lately appeared for a short time at the village of Chiroee, ten miles nearer. It also rages at present in the large town of Rhadanpore, and has extended its ravages to the province of Sind, where it is said to be the cause at this moment of great mortality.

" One case of the disease occurred in this town: the person affected came from Buchau, and died the day after his arrival; but the precaution being immediately taken of having all those who had had any communication with him, sent out of the fort, and his house fumigated and untiled so as to expose it to the sun, it did not again make its appearance. Another instance occurred of an infected person dying outside the fort, all communication with the places where the disease prevailed having been prohibited.

" Such are the various places in which this disease has raged, or where it still continues to exist; and until accounts are received to disprove the opinion, however curious the fact, it must be considered to have originated in Wagur. No affection of a similar kind can be traced to have previously existed in any country bounding that province; on the contrary, it appears to have spread from Wagur as from a centre, and from Kuntakote as the centre of Wagur.

" No case of this disease has come under my own observation. It appears from the description of the natives, to be ushered in by slight fever, attended with great languor, prostration of strength, loss of apetite, and a certain listlessness or indifference as to the result, which is perhaps as much owing to the apathy of the native character under such circumstances, as to any peculiar effect of the disease,—swellings of the inguinal and axillary glands, or of both, soon make their appearance but at no particular period, and as these come rapidly forward to suppuration, or the contrary, so is the chance of recovery greater or less. Death takes place at all periods, but generally from the third to the ninth day.

" This disease attacks people of every age, and even infants. It appears principally to prevail among, nay, to be almost exclusively confined to the more indolent classes of society, and particularly those who live on vegetable food: hence the Hindoos, and especially those of the Brahman and Banyan castes, have suffered severely.

" In the town of Moorvee, where there are nearly 400 Rajpoots of that caste which eats animal food, and who are in the habit of drinking spirituous liquors, and indulging in the use of opium, not one has been affected, though the disease has been raging in every direction around them; and a similar observation, I have heard, has been made in other places.

"Another remark which the natives make, and which I think is likely to be just, as they are not apt to take notice of any thing which is not extremely obvious, is, that those engaged in the expression of oil are not liable to infection.

"Such is the information I have been able to obtain from the natives, as to the principal symptoms and character of this disease. Mr Robeson who accompanied Colonel Barkly's Brigade into Wagur, and who went to one of the infected towns, and reported on the nature of the disease to the Political Agent with the Force, confirms the description I have given; and further adds, that the disease was accompanied with a weak and irregular pulse, often with low delirium, and that the bowels were in general constipated. In many cases hemorrhage from the lungs took place, which was almost always a precursor of death.

"Mr Robeson, from what he saw of the disease, did not consider it as contagious, but as depending on a certain state of the atmosphere; the people of the town appeared to be of the same opinion, and did not think they ran any risk of infection by their intercourse with the sick.

"With regard to the mortality occasioned by this disease, it has from every account been very great, wherever it has prevailed. Very few of those affected appear to have recovered, and the proportion of deaths, compared to the number affected, has been perhaps as great, if not greater, than in any epidemic or contagious disorder that ever appeared in any country.

"At present the disease, from all accounts, has abated, both in Buchau and Moorvee, the only two places in which it now exists, either in Kattywar or Wagur; but this is perhaps as much owing to the great mortality it has occasioned, having left fewer subjects for its operation, as to any diminution of its virulence. It appears to be at present spreading entirely northward, having made its way, as I have before mentioned, to the city of Rhadanpore, and the province of Sind; while I have heard of no place to the south of Moorvee where it has yet appeared.

"Though we have no certain information with regard to the origin of this disease, yet, if we take into consideration its symptoms and character, as I have described them from the accounts of the natives, and which have been confirmed by a most intelligent medical gentleman, who had an opportunity of examining many of those affected; and at the same time, if we reflect on the dreadful mortality it occasions; we will be led to consider it as the same disease which, under the name of the Plague, almost annually prevails in some other countries of Asia, and in Africa.

"It exactly resembles that disease in its symptoms, and also appears to me, from every account, to be propagated by contagion. How it may be conveyed from one place to another, has been completely exemplified in the case which occurred in this town, where if proper precautions had not been taken, I have no doubt it would have rapidly spread among the inhabitants. No disease either, of a similar kind has ever been known or heard of in this country, and this is also, I think, no small confirmation of this opinion.

" Having taken all these circumstances into consideration, I early communicated my opinion of the nature of the disease to the Resident in Cutch, who immediately adopted such precautions as were necessary to prevent its introduction into this town, and which I have no doubt will answer the purpose intended. As this is also an intermediate stage between the affected places and the large seaport towns of Moondra, and Mandavee, (which have an extensive commercial intercourse with almost every place on this side of India,) our success in preventing the admission of the contagion into this place, will, in all probability, be the means of saving them from infection; where, if the disease were once to gain a footing, it would be impossible to say to what extent it might be propagated.

" The disease as yet has made its way to none of the sea-port towns in Cutch, nor, if it continues to abate in Wagur, is it likely to do so, as they are so far removed from the seat of its operations. Whether or not it has reached any of the bunders on the Indus, there are no certain accounts.

" Since writing the above letter, information has been received that this contagious disorder is now raging with great mortality in Hydrabad, the capital of Sinde. A letter from that place mentions, that the daily number of casualties amount to 60 or 70."—Pp. 185–188.

From the foregoing observations, it results that Mr M'Adam regards the distemper which prevailed in 1815 and 1816 in Kutch and Kattywar, as the same as oriental or glandular plague, both in symptoms and in mode of propagation, which he considered was by contagion.

The distemper described by Mr M'Adam was epidemic in Kutch and Kattywar in 1815 and 1816. Next year a distemper very similar in its symptoms and effects appeared in the Zillah of Ahmedabad, and continued to extend over the towns and villages of the district during the two subsequent years, viz. 1818 and 1819; and in 1819 and 1820, a distemper quite similar appeared in many parts of Kattywar, and once more in several of the Zillah of Ahmedabad.

The Zillah of Ahmedabad is an irregularly shaped district of moderate size, situate at the upper or northern extremity of the gulf of Cambay, and towards the east of the large province of Kattywar. It is so denominated from its principal town. The province or district of Kattywar is part of the large province of Guzerat, a large trapezoidal space of country, which is situate between the gulf of Kutch on the north-west, and the gulf of Cambay on the south-east. These two gulfs run so far up into the country that they make of this space of land a sort of peninsula, at the sea-coast end of which is situate Kattywar, and at the neck or land extremity of the same the Zillah of Ahmedabad. The name of Kattywar, indeed, is applied by the natives chiefly to that portion of Guzerat which is most completely peninsular, and is situate between the bays of Kutch and Cambay.

In this district, according to Mr Gilder's inquiries, the distemper in question made its first appearance in the year 1817, three years after the time that the provinces of Guzerat and Kattywar had been most severely ravaged by famine. The first place where it appeared was Dhollera, a town on the Runn of Kutch, not far from the influx of the Bhadur; and it was added that it was brought thither from Morevee, a town in Kattywar proper, where it was already said it had been prevailing in May 1816. On this point, however, of transference or importation of the distemper, different opinions are entertained.

In Dhollera only three people are said to have suffered; but the whole three died. From Dhollera it spread to the neighbouring villages, viz. Buriad, Rajka, Peeplee, &c. and reached Dundooka, one of the chief towns of the Zillah. In this neighbourhood it continued till the close of 1817, destroying many persons in the villages bordering on the Runn. In 1818, its attacks were less frequent, but it did not wholly disappear; for insulated cases were frequently occurring in the villages of the Dundooka and Limree districts. Limree, it may be added, is a considerable town in the Zillah, in Kattywar, not far from the north-western shore of the gulf of Cambay.

In this manner, the distemper continued smouldering till the beginning of 1819, when it broke out in April furiously at the village of Buriad, situate three miles to the westward of Dhollera. There it raged till the commencement of the monsoon, and in two months it destroyed entirely forty families. In the beginning of June 1819, it spread to Rajka, a village three coss to the west of Buriad, and nearly depopulated it. During the rains, it spread over an extensive part of the country, destroying certainly whereever it went.

In the end of June 1819, it appeared in Limree, and destroyed in a very short time, it is calculated, from 1500 to 2000 of the inhabitants, after which the survivors deserted the place for a time altogether. The distemper then abated in violence in the town, apparently from want of subjects; but it broke out with increased force in several towns and villages to the east and west of Limree, depopulating several entirely.

Mr Gilder further remarks, that during the whole of this havoc among the natives, the rains were pouring down in torrents; and, as the villages are mostly situate on rising grounds or hillocks, while the intermediate spaces were flooded, the whole surface of the country presented the appearance of a sheet of water.

The disease did not at this time extend to the eastern bank of the Runn.

It appears to have been greatly prevalent and fatal among those castes who subsist chiefly on vegetable food.

The distemper appeared in Moolee, and Sila, and other places, near Morevee, in 1820, and, as usual, destroyed a considerable number of the inhabitants of each. Moorba, a village situate on an elevated and airy position, was the only place where the inhabitants were not attacked, though daily and hourly communication was maintained between its inhabitants and those of Morevee and Moolee, and other towns in which it was prevalent.

The symptoms of this distemper were, on the whole, very similar in all the places now mentioned; violent and overpowering febrile symptoms, followed by swellings in the lymphatic glands, with delirium and coma. The following sketch is given by Mr Gilder:

"From the native descriptions of the symptoms of this malady, one might be led to infer the existence of two distinct diseases: this supposition might at first seem corroborated by their having separate names for each.

"The one they term Ghant ka rog, or the knotty disease; the other they distinguish by the appellations of Kokla ka rog and Tao ka rog, signifying the expectorating disease and the fever disease: these, though equally severe in their characteristic symptoms, and similarly fatal in their effects, may only be aggravated varieties of the same disorder, acted upon by a vitiated atmosphere, and other local causes, which could only be successfully developed by personal investigation.

"They describe the Ghant ka rog to possess the following symptoms: great and general uneasiness of the frame, pains in the head, lumbar region, and joints, on the day of the attack; hard, knotty, and highly painful swellings of the inguinal or axillary glands (whence the name) appear; in some instances, the parotids are affected, and in four or five hours fever supervenes. These symptoms go on increasing in violence, attended with great thirst and delirium, until the third day of the attack, when death closes the scene; but should the patient survive the third day, hopes of recovery begin to be entertained. Suppuration of the glandular swellings occurs on the fourth or fifth day, and the other symptoms gradually diminish in force; the fever assuming a milder aspect, and the patient regaining his strength in twelve or fifteen days. Such favourable terminations are, however, rare.

"This form of the disease was introduced into the village of Bullole in the end of December last, by a Banian of the place, who had returned from Dewpoora, whither he had been on business, and where it was raging at the time. The night after his return he complained of great uneasiness, pains in his loins and joints, attended with fever: these symptoms continued increasing throughout the night, and the next morning he perceived swellings of his inguinal glands, which were exceeding painful: during the day he became delirious, and complained of intense thirst: no abatement occurred on the third day, on the evening of which he died.

"Every person belonging to this man's family, both male and female, died within the space of ten days, of the same disease, which spread to the other two Banian families in the village, and committed similar ravages. After remaining about fifteen days from its first introduction, during which period it destroyed about thirty of the inhabitants, out of a population of nearly 200, it disappeared.

"The following case occurred in May 1817 at Dollera, while I was there on business. The symptoms and treatment are stated from the best of my recollection, and, though the disease terminated fatally, I am inclined to think that bleeding and the use of brisk cathartics in the early stage, would go a great way in arresting the progress of this destructive malady.

"In a lad 14 years of age, the disease was ushered in with considerable pains in the head and loins, sickness at the stomach, and vomiting of bilious matter, turgidness of the eye, full, strong, and hard pulse, skin dry, and intensely hot; I immediately gave him a strong cathartic of jalap and calomel, and took away twelve ounces of blood: the symptoms subsided in two or three hours after the operation of the medicine, but returned with increased violence the following morning, when I repeated the medicine and bleeding; this partially relieved him: an exacerbation of all the symptoms took place in the evening, with an enlargement of the glands in both armpits, which continued to increase throughout the night, but without assuming any circumscribed appearance as in common bubo; he died at sunrise the third morning from the attack.

"This boy was living in a small hut, surrounded by his relations, who, of course, performed all the requisite duties his situation required, but not one of them was affected by the diseased.

"Of five other cases, the first symptoms yielded to brisk cathartics. In the same compound where these cases occurred, there were upwards of 200 people including children, all of whom came from the same district.

"The other form of the disease, known to the natives by the names of Kokla and Tao ka rog, and which at present prevails most, is described as exhibiting the undermentioned symptoms: high fever, attended with burning and excruciating pains about the scrobiculus cordis, skin intensely hot, with a feeling as if the body within was on fire; hic-cough with deep and oppressive breathing ensue: there is a pricking sensation all over the body, as if it were perforated with pins; considerable pains in the chest and joints, and about the navel;

delirium, great anxiety, and thirst follow: at length the patient hawks up clots of blood, the difficulty of breathing increases, and death generally occurs on the second day of the attack.

"In both cases, native practitioners administer stimulants for the first, and astringents for the ultimate stages of the disease: but, being averse to the use of purgatives, (which experience teaches us are the principal and only effectual remedies prescribable in the fevers of India,) their labours are unattended with success."—Pp. 195–198.

The description of the disease in Morevee, Moolee, Sila, and Wunkaneer, by Mr Thomas Whyte, is very similar:

"The town of Morevee is a large place, surrounded with very high walls, situated pretty high on the bank of a fine river. Neither in it, or its neighbourhood, do there appear to exist any of those causes which are understood to produce the common intermittent fevers of this country; and the character of the place has in general been remarkable for health. The whole space within the walls is, however, crowded with houses, and although the population at present is very thin, they were probably filled with inhabitants before the place was visited by famine and sickness.

"The disease which is the subject of this paper, distinguished all over this district by the general name of Ghant, (and which from the exact similarity between the symptoms of that now raging at Moolee and Sila, and those described as the common appearances in 1817, is unquestionably the same disease,) prevailed for five months in this town, three years ago. It began in the cold and lasted through the hot months, without undergoing any change from the change of weather. It is in general stated to have carried off 1500 people, which account, I should think, is exaggerated about two-thirds above the actual number. It must remain a subject of conjecture, however, for the different statements vary so much, as to set all accuracy at defiance. The same doubt exists regarding the proportion between the deaths and recoveries; some declaring that one in ten recovered, others that not one in fifty did so; and a barber-surgeon of the town informed me that 2000 recovered, and 2500 died. He said he never meddled with the patients till after the fifth day, when it is probable the principal part, or whole of the danger was at an end. His practice consisted entirely of hot stimulating applications to the buboes; he gave no internal remedies.

"I saw and examined four of the recovered men, which number, although so small, seemed to be procured with difficulty, and may serve to show how few actually survived. In the houses of the three first, no other person had been affected; in two there was no more than one bubo; the third had three; and of the whole only one suppurated. The fourth was a Mussulman who took care of cows, and sold milk and ghee; his house, from his description, was very close and ill-aired, and of nine, composing his family, seven had been attacked by the same disease; he was first taken ill, two were seized two days after, and the rest successively at about an equal distance of time. Three died, and four recovered. They all had first, a severe fever, which deprived them of their reason on the second day. In all who recovered, the consequent debility was very great, and convalescence extremely slow. None of these four men entertained the smallest suspicion of contagion. One said, he could not tell how he caught the disease, unless it was from his wife, to whom he had been married about six months before, and who then had a little itch upon her thigh. He seemed to puzzle himself very much, however, in accounting for his illness, being not at all sure that his wife had been the cause of it; and the other three could assign no reason whatever. Indeed, no one in the town considered the disease as contagious, and the Thakoor, or Chief's brother, seemed to have proved very satisfactorily in his own person, that it was not so. He, with a laudable kindness and anxiety for the welfare of his people, had been in the habit of frequently visiting the sick, comforting them, enquiring into the nature of their complaints, handling and touching them, and all with complete impunity; from which, I think, we may safely infer that casual intercourse is unattended with much danger. It is probable, likewise, that the disease is not readily communicated by the touch, and possible, that confinement to the same apartment is, in general, necessary for its production.

"The only part of the town which did not suffer was the Thakoor's palace. It is a very large, well-aired place, elevated above the other parts of the town in a slight degree, and contains about 100 people, all of whom enjoyed a perfect immunity. On the opposite side of the river, less than a quarter of a mile distant, on a very high and exposed situation, is a small village, called Moorba, not one of whose inhabitants was attacked, although a daily and hourly communication was maintained during the whole period of the above five months; the people say that the river set bounds to the disease. On the same side of the river with Morevee, disease was communicated to a village two coss off, called Moolee; but the situation of this village is low, close, and ill-aired, being surrounded with jungle. It is more probable, therefore, that Moorba owed its exemption to its high and airy site, and that Moolee suffered from the opposite causes.

"Wunkaneer is a large, walled town, situated at the foot of a mountain of considerable height, and on the bank of a very fine river, distant about eighteen miles from Morevee. While this disease raged at the latter place, it likewise paid Wunkaneer a visit, and the people say that it came from Morevee; it lasted no longer than fifteen days, and was almost solely confined to one class of people, viz. the Boras, whose occupation is the manufacture of cotton cloths. I am told that the greater part of the cotton was imported from the surrounding country, and part of it from Morevee. The number of deaths was estimated at sixty among the boras; and no more than four or five of all the other inhabitants suffered. These numbers, too, are probably correct, for the testimony of different people was nearly the same; and it is obvious that where the disease was so nearly confined to one set of people, it would be no difficult task to arrive at precision. Every one who was attacked is said to have died. The boras became so much alarmed at a visitation so dreadfully severe, and at the same time so partial, in which every one of them seemed to have been doomed to destruction, that they all left the town, and went to live upon the mountain in the neighbourhood, leaving only those that were sick in their houses, and one attendant upon each of them ; the whole of these very soon died. Whenever they heard of a death, the friends of the deceased came down and performed the last offices as speedily as possible, returning again to their abode in the mountain. Some of those who attended these funerals were taken ill, but not many could have suffered in this manner, for at the end of fifteen days after their removal to the mountain, the disease had entirely disappeared from amongst them. When this was the case, they all came down and occupied their houses, and they declare that after this, no person was attacked with the disease. This is a curious and singular fact, affording a rare instance of a measure dictated by fear, and carried into effect, without reason or reflection, being attended with complete success. In fact, it corresponds very nearly with what has been recommended to be done by one of our best writers, Dr Adams, on such an occasion; and is a very good practical illustration of the measures which, with so much care, deliberation, and strict induction, from the facts before him, he laid down, and thought likely to be successful.

" In the town of Sura, I was told that a travelling merchant had come thither, and died after six days illness, from a fever accompanied with buboes. His wife had not been affected, and left the village shortly after. After the lapse of a month, a woman of the caste that takes care of cows, was seized with the same disease, and died in six days. She lived in a different part of the village, to that where the merchant died. Eight or ten days after her death, a woman of the same caste, and living in the same compound, was likewise seized with this disease, and died on the third day. Her death had occurred about twelve days before the time of my visiting the village, and up to this period no other person had been attacked. There were a great number of people living in the same compound, and both women had a number of children. The last who died had suckled an infant till the time of her death.

" Then a Banian's boy who had come from Moolee, which is twenty miles distant, was here attacked with this disease, and died, but did not communicate it to any other person. The people of the province say that this year the disease appeared first in Limree, afterwards in Wudwaum, from which it came to Sila and Moolee. I visited the two last named places.

" Moolee is a large and very populous town, the inhabitants being nearly all Rajpoots. It is surrounded with an old ruinous wall, full of breaches, which are all carefully stuffed with thorns: and every house has a wall of the same materials, much higher than a man's head. No better means could possibly have been adopted, completely to exclude ventilation, if such had been the sole object of the inhabitants; and it might fairly be made subject for dispute whether in shutting out their enemies, they have not shut in one far more powerful than the whole of them together. It is situated on the bank of a river, even now almost dried up, and the water of which is perfectly brackish.

" The disease had carried off in one month about eighty people, and was still prevailing. I saw a great number who had buboes without any fever, and was told that upwards of 120 had suffered in this way; these people walked about without either alarm or inconvenience, for none had died, and not many of the buboes had suppurated; not one, however, with fever had survived.

" There was a description of cases here, distinguished by fever, and hemorrhage, apparently from the lungs, without buboes; these were reported to be as surely, and even more speedily fatal than those with fever and buboes. This had been the most common form of the disease at first, but lately had rarely appeared. Fortunately, one case came under my observation; I say fortunately, for I had frequently heard it described both at Wunkaneer and Morevee, without being able to understand what was meant; every narrator so mixing his account of appearances and actual facts with what he supposed to be their causes, and with the internal changes, which he supposed to take place, and immediately to produce death, that the descriptions became involved in a cloud of utter confusion.

" In this man the heat of the body was not much increased, nor the pulse greatly accelerated; his bowels were not disordered, nor did his tongue indicate much febrile irritation; he was able to walk about and converse, answering questions distinctly; no person would have thought him in danger; but there existed in the patient's mind a degree of alarm and anxiety altogether disproportioned to the apparent symptoms; he had only been attacked that morning, and all consideration seemed absorbed with a pain in his chest; he answered to my question, whether he had not other pain, as in his head, his back or limbs, that these were slightly painful; but he immediately recurred to his chest, dwelling upon that with a look of the most painful distress, and if not questioned about other symptoms, it seemed as if he would not have mentioned them. He had besides a very slight cough; so slight that it might easily have escaped unnoticed, and it was accompanied with a discharge of blood from the mouth. The following day he was delirious, had a burning skin, with a very quick pulse; I searched for but found no buboes. He died in the course of the succeeding night, *i. e.* in less than forty-eight hours from the first attack. The characteristic symptoms of this variety are slight cough, pain of the chest, and hemorrhage from the mouth, attended with fever but no buboes.

" I saw four other cases of the disease, all of which were marked by a low typhus fever, buboes in the groin, or armpit, (and in one case, in the seat of the parotid and submaxillary glands of one side.) In all there was the greatest debility; and in one, a universal tremor of all the muscles of the body, giving a very singular appearance. All complained of great thirst; the tongue was covered with a white fur in the centre, through which, and at the edges were seen the papillæ of a shining fiery red appearance. In none were either petechiæ or carbuncles to be seen, and I may observe, that I have neither seen nor heard described, either of these symptoms, nor any other cuticular affection; petechiæ might easily escape common observation, carbuncles could not possibly do so In general, there is not more than one bubo, although in one of the above cases where only one was visible, I distinctly felt hard, knotty swellings in both armpits, and in the opposite groin.

"All the above cases, as well as those of two females of rank, which were not shown to me, terminated fatally, during the first and second days of my visit, and no other cases occurred during my stay.

"They had not the slightest opinion in the place of the disease being contagious; indeed, they were determinately of opinion, that it was not so. One man, the thakoor's brother, confidently assured me that contagion was not the cause of the disease, affirming boldly that no man could be a better judge of the question, for that none had greater experience, he having lost in his own family, eight women and one boy. 'No, no,' said he, 'oorta joode;' contagion is quite a different thing. Another woman died in this man's house while I was here, making a total of ten in his house.

"Nothing can show more clearly the evil arising from bigotted prepossession than the case of this man's family; for it is difficult to conceive what he would have considered as satisfactory proof of the reality of contagion. To the unprejudiced observer, the probability will appear very strong, that these women caught the disease one from the other, and that the boy got it likewise from them; or, which is the same thing, that the atmosphere of the apartment where they lived, being rendered infectious by the effluvia of the diseased, proved afterwards the source of disease and death to those who were living continually under its influence. These were women of rank, and the seclusion to which they have been doomed by the force of opinion and custom is well known: it operated so far as to prevent my seeing the two women mentioned above as having died, one of whom belonged to this family.

"They sometimes said that this was no disease, but an order from the Almighty to remove whomever it attacked; therefore, probably, all resistance was thought to be wicked or vain.

"Sila is a large walled town in the neighbourhood of an extensive tank, which supplies excellent water. The disease had prevailed there about two months and a-half; at first it had been very severe and general, but lately it had become more partial in its attacks. The same class of people, viz. the Boras, that had suffered so much at Wunkaneer, were likewise the first attacked, and had suffered in a proportion greatly beyond that of the other classes; 120 were said to have died, of whom sixty were Boras. In some particular houses the loss had been very great; some losing five, and others eight or nine of their inhabitants. At one time, they thought the disease contagious, for people returning from a funeral had been suddenly attacked and carried off, but lately it had lost in their opinion this character. One of the houses said to have suffered particularly was an oilman's; five people died in it. I was told in Moolee, that one man of the same profession had died; the total number of people of this description living in Sila is said to be fifty; in Moolee thirty.

"I was here shown the cases of four people, in advanced stages of the disease; they were the only survivors, and only one appeared to be in any danger, or rather in any doubt, for in her case even, the danger appeared to be nearly over.

"She was in the ninth day of the disease, had a bubo in the upper part of each thigh, both very painful to the touch; they appeared on the second or third day of a fever which had deprived her for a time of her senses: even at this time, her mental powers were so weak that she found it difficult to command attention to any subject, and her memory seemed frequently to fail her; she was exceedingly weak in body as well as mind; her eyes had a glassy appearance, and one was partially suffused with blood. A very severe purging had just terminated after some days duration; she had likewise suffered much from copious and universal sweats; but both these symptoms from the period of their appearance, seem to have been the prelude to improvement. She is ignorant as to the cause of her disease; it was not in her immediate neighbourhood at the time, and has not apparently been communicated to any other person. She lies in a veranda, the front part of which is closed up with a wall of Kirby stalks, and several calves, as well as men, women, and children are her companions in this apartment.

"A Banyan's wife had just died in the town, on the ninth day of her illness; she had a bubo on one side of the neck, the inflammation and swelling from which had extended over to the other side, appearing to occupy the whole throat. Her brother had died a little before her, in the same house, on the third day. There were no fresh cases at present in the town, and the disease was thought to be on the decline.

"On its first visitation, that form noticed above as distinguished by fever, slight cough, pain of the chest, and hemorrhage, had been the most common; but it had disappeared for some time. This variety seems to have appeared first at every place which has yet been visited. I may remark, that the famous plague of Athens is said to have been ushered in by an epidemic catarrh or pleurisy, which yielded to no remedy, and which gradually gave way to a fever, accompanied by the true pestilential tokens. In the town of Sila only seven or eight are said to have had tunda ghant, or buboes without fever.

"Before proceeding farther, it seems necessary to take notice of some peculiarities of the people in this province, which, in my opinion, exert a powerful influence over the progress at least of this disease. They are the only people whom I have seen in India, who might properly be termed filthy. Ablution, which, from being sanctioned and enjoined by divine authority, in every other place is practised with all the diligence and scrupulosity of their other religious ceremonies, in Kattywar, from what cause I know not, is almost wholly neglected. The evil consequences of this neglect are much increased by the enormous quantity of clothes which it is the fashion for all ranks to wear; and in most ranks these clothes are but seldom changed. In some of the inferior it seems almost as if they were allowed to rot upon the persons of their owners, without once being washed, or even removed from their first being put on, till they can no longer serve the purpose of covering their nakedness. Another common practice in most parts of India, friction of the body with oil, which might in some measure have counteracted the evil consequences of the other neglect, is unfortunately here also in equal disuse. This very grateful and salutary practice, which, by encouraging, and at the same time modifying, the process of perspiration, has so powerful an effect in regulating the temperature of the body, as well as in preserving the balance of the circulation; and by preventing cuticular absorption, must prevent the admission of contagion into the system in that direction, has, upon very good authority, and extensive experience, been supposed a complete preventive as well as a powerful agent in the cure of plague. If this disease is plague, some of the facts above related are sadly at variance with this opinion. I hope, however, I may not be accused of giving way, too far, to prepossession on the present subject, if I declare that my opinion of the efficacy of this practice has not been much shaken by them. The facts were not witnessed by, but reported to me, and I had dwelt so much upon the necessity of ablution and oily friction in my conversation with the inhabitants, that I shall not be much surprised if, when I inquired regarding the oil manufacturers, they had invented these circumstances to excuse to themselves and me their own apathy, and neglect of the precautions which I had pointed out. If this disease is communicated by the touch, there can be no more powerful antidote than oily friction; but where infection is received with the breath, it will be efficacious only in so far as it invigorates the general system, and enables it to resist the influence of disease.

" In addition to the above causes, which are permanent, it is likewise certain, that for some time past, the great body of the people have laboured under all the depressing disadvantages of precarious existence.

" The severe famine which visited this province gave an influence to the above causes, which otherwise they would never have acquired; the one thus aiding the other, both gained an increase of force. Indeed, pestilence and famine have so constantly gone together, and are so linked oven in our speech, that a sentence seems incomplete where one is pronounced without the other.

" The consequence of all this has been general distress, general despondency, and general inactivity; every town swarms with idlers, than which no more fertile source of disease exists; its effects are so powerful, that they may be traced even in individual instances; but where indolence and inactivity affect a whole community, the consequences then force themselves upon our notice.

"In Kattywar, industry is unknown, (in many places it is beginning, however, to revive,) and that chiefly in the districts where this disease has raged. Cultivation is almost neglected, and the inhabitants depend for the means of subsistence principally upon the milk and ghee furnished by their cattle. Partly from this cause, and partly from the strictness of their religious tenets, which rigidly forbid the death of any animal, every family has accumulated a large stock of cattle; and these, amounting to hundreds or thousands, according to the size of a town, are invariably driven within the walls at sunset, and are penned for the night in the same house, which contains the rest of the family. Thus no limits are set to the number of cattle accumulated in every family, excepting by death, the consequence of disease, or old age.

"Still all animals are doomed to die; and, notwithstanding the humanity of religious tenets, this forfeit must be paid in one way or other. In proportion, therefore, as we diminish the number of deaths by artificial or violent methods, we should naturally expect an increase to the number by natural causes; or, in other words, that disease and sickness would be multiplied. Accordingly, we find this to be the case; the cattle of Kattywar afford the clearest proof of it; few seem altogether free from disease, and many are seen in the most wretched condition, crippled, and crawling about with swelled legs and diseased skins.

"The degree of obscurity which hangs over the origin of most fevers, can seldom be removed by years of the best directed inquiry. Thus after so many years of observation and experience, during the whole of which the eyes of an enlightened public have been diligently directed to the subject; the causes which produce the annual fever of North America is still a matter of doubtful discussion. The same remark is applicable to the yellow fever of the West Indies; some still attributing it to contagion, others entirely to local causes.

"It may perhaps facilitate our inquiry into the cause of the present disease distinctly to point out what at least have not been agents in its production. We may then at once, and altogether exclude from consideration all putrifying animal and vegetable substances, all effluvia from jungles and marshes; for every one of the towns that I have visited are in a remarkable degree exempted from every thing that could produce these; their situation being either on the rocky bank of a running stream, or on a dry sandy soil.

"Leaving these causes, therefore, out of sight, we may now observe, that another cause of fever, at once the most common, and the most destructive, has been clearly and indisputably ascertained—I mean the cause of infectious or typhus fever.

"We know that wherever a number of human beings are crowded, and confined within a narrow space, where the atmosphere which they breathe, cannot frequently be renewed, where the effluvia from their bodies remain undiluted, are allowed to accumulate, and attain a sufficient degree of concentration, then the atmosphere which is loaded with them, will excite in those who live within its influence, a fever of the most malignant nature.

"I am more inclined to believe that the disease was imported into this country, and with this belief that of the people in general coincides. There appear to me to be a great many facts which powerfully support this view, and which could not satisfactorily be accounted for in any other way. It is not known to have attacked any two places simultaneously, however much alike in those local causes, of which we have already admitted the probable influence; after appearing in one town, it went, after a little time, to the town nearest, as from Limree to Wudwan, Sila, and Moolee this year, or from Morevee to Wunkaneer in 1817, affecting slightly the intermediate villages.

"The class of people first and principally affected, both at Sila and Wunkaneer, affords an additional presumption, that in these instances, the disease was imported; for, it is in every way probable, that the cotton was the vehicle by which the contagion was conveyed. The sudden disappearance of the disease from Wunkaneer, upon the separation of the healthy from the sick part of the people, cannot be satisfactorily accounted for by any other supposition than that of imported contagion.

"If local causes had produced the disease, why was their operation not evident again after the return of the boras to their houses, for, in so short a space, no improvement in their condition could have taken place, unless a diminution of their numbers be considered as one? Under such circumstances again, the fact of the disease disappearing and reappearing after the lapse of years, cannot be accounted for. It is probable, that during the period of the disease being absent, these local causes remained much the same; but, on the other hand, if we believe in imported contagion, the difficulty is at once removed.

"If we consider the nature of fomites, we shall at once understand how they may lie inactive for any length of time, and then, probably by some accident, be at once brought into action: it is well known that fomites, so far from losing their power by time, will, if confined, acquire a great increase to it.

"We may the rather wonder that the disease is so seldom produced, or so seldom conveyed from one country into another: this, indeed, is a mystery which it is impossible to solve. Quarantine alone (considering the careless manner in which goods are ventilated) is insufficient to account for the long exemption which England has enjoyed.

"In Bombay, where none is practised, although a constant intercourse is maintained between that port and the Red Sea, how shall we account for the disease never having been imported?

"Quarantine, I am informed, was enforced in the town of Dundooka this year; notwithstanding which the disease made its appearance there. However, as it is probable that the authority was not sufficient to prevent the introduction of goods, (however easily people from the infected towns might have been excluded) no inference can fairly be drawn from this single instance.

"In the meantime it is certain that the disease has been introduced into many villages and even towns of a considerable size. (Than is a large walled town, and otherwise favourable for its progress,) without having spread.

"From all that has been said, I think, it will be apparent that although this disease in a close ill-aired situation is highly contagious, in an open space it is quite the reverse. It has never remained long in any except the large and walled towns, where probably more or less of infectious atmosphere always exists; while the town of Sila suffered so much, the Dherwaree, which was outside the walls, was free. The thakoor's house, (the only one in the town well-aired,) at Morevee, afforded safety to 100 people, while the disease lasted for five months in every other part of the town.

"The town of Moorba kept its inhabitants in safety for the same reason; and all the villages in the district may be said to have been safe, for the disease never spread generally among their inhabitants; and lastly, when, from fear, the sound part of the people were separated from the diseased, the disease which, while the boras remained crowded in their dirty huts, within the walls of Wunkaneer, threatened their entire destruction, was immediately and entirely removed.

"No doubt can exist that the disease was propagated by contagion in the house of the thakoor's brother in Moolee, where, in one family, nine women and a boy died from it; or, in the house of the boras in Morevee, where seven out of nine people, living in a dirty confined house, were successively attacked by it.

"It would perhaps have been better if I had at an earlier period said, what I now intend to do, concerning the nature of this disease, as I have in many places been compelled to anticipate so far, as to speak of it by a name, which I have no hesitation in at once assigning to it, viz. Pestis.

"Doubtless in former times all severe and fatal fevers were too indiscriminately classed under, and handed down to posterity, with this denomination: on the other hand, it has been the error of physicians more frequently in latter times, altogether to doubt the existence of this disease, until its ravages had fatally and too surely pronounced its nature.

"There is something so repulsive in the name of Plague, that our wishes naturally beget a disbelief of its reality; let us be cautious how we feed a belief which may be fraught with such dangerous consequences to hundreds of our fellow-creatures; but equally free from prejudice either way, let us dispassionately call in the aid of nosological definition to guide us through the difficulty. It is I believe allowed, that, although a great many more symptoms frequently attend it, (and no disease has varied so much in various epidemics,) yet that Cullen's definition includes all that are strictly pathognomonic—'Typhus Maxime Contagiosa cum summa debilitate; incerto Morbi die, eruptio bubonum vel anthracum.'

"All these symptoms are present in almost every case of this disease, and are mentioned always by the natives as distinguishing it. The very name which they have given to the disease, Ghant, means literally a bubo. It is, indeed, true that petechiæ and carbuncles, if ever, are but seldom seen; symptoms commonly attendant on Plague; but we may observe, that petechiæ are not mentioned by Dr Cullen, and that he mentions carbuncles in an ambiguous and guarded manner, 'Bubonum vel Anthracum,' which by no means implies their constant and necessary presence.

"It may be urged, likewise, that in one particular form above described, no buboes are ever observed; but we learn from Dr Russel, that the same thing was very common at Aleppo; and he seems to think that their non-appearance was owing to the rapid termination of these cases in death; this is extremely probable, for these eruptions do not come out critically on a particular day, but '*incerto morbi die*;' and in the prognosis of this disease, the later they were in appearing, it has always been observed, so much the greater has been the danger, and *vice versa*; where they come out before the fever, the danger is seldom great. The same thing has always been observed regarding the yellow colour of the skin in yellow-fever, where the patient dies very early in the disease, the yellow colour not having appeared. Buboes are the grand distinguishing symptoms of the present disease, and, if the patients were to live long enough, would, doubtless, invariably make their appearance.

"I apprehend, therefore, we shall be as little justified in refusing it a place under the head of plague, as those authors have been who have been so eager to class all malignant or putrid fevers under that name, merely because they were generally fatal, and buboes were sometimes, though rarely and accidentally, observed to make their appearance in them.

"I feel it unnecessary to pursue this subject farther, than just to remark, that all the other symptoms which have been enumerated as frequent in plague, will be found in the different cases of this disease; and, if it were fair to infer anything from its fatality, in this feature it might justly claim pre-eminence of most of those epidemics of which we have a history.

"I was told by the Thakoor's brother at Moolee, that some of the women mentioned above were suddenly taken ill in the midst of their usual avocations; that while conversing in the middle of a sentence, they would fall down, blood come fron their mouth, (not in any great quantity,) and death immediately follow. After allowing a sufficient portion of this account for exaggeration, enough will still remain to show how suddenly fatal sometimes its attacks have been.

"The symptoms in general were such as denoted rather nervous debility, as convulsive tremors of the muscles, inability to walk or stand after the first day, delirium, or wandering of the mind, and a very weak pulse; than those which have been supposed to indicate a putrescent state of the fluids. The stomach and bowels were not remarkably deranged, there was no fœtor of the breath, or excretions, no sordes upon the teeth, and the tongue was only coated with a white fur; hemorrhage was the only symptom which could be supposed to arise from a putrescent state of the fluids. In women with child abortion followed attacks of this disease, and a dead child was produced.

"I shall avoid the subject of cure, having no fact that could at all illustrate it. At Moolee I gave a little medicine to two patients, who died, as all the others had done before them; notwithstanding which, it evidently prejudiced the mind of the people against me, (I believe they thought me invested with little less than divine power on my first arrival,)and it operated so much in my disfavour at Sila, whither the news had travelled before me, that when I first arrived they obstinately refused to let me see any of the sick."—Pp. 155—182.

Upon the nosological position and pathological characters of the two forms of this disease, the *Ghant ka rog*, and the *Kokla ka rog*, it is not very easy to come to a decided opinion. Mr M'Adam thinks that the history of its symptoms, and its origin and progress, as well as its dreadful mortality, justify the inference, that it is the same as the glandular and anthracoid plague of the Levant; that it is propagated by contagion; and that its introduction and diffusion in any community may be positively and certainly prevented by exclusion of the sickly and infected, seclusion of the healthy, and segregation of both.

Mr Gilder, on the other hand, says of the disease described by him in Ahmedabad, that he thinks the name of plague has been erroneously applied to it; and expresses the opinion, that it is an aggravated form of typhous fever, rendered more terrible in effects from local causes, and a morbid or unhealthy state of the atmosphere.

The opinion of Mr Whyte inclines, in some degree, to that of Mr Gilder; but he also expresses the opinion somewhat confidently, that it belongs to the head of plague. Mr Whyte mentions, that we must, in accounting for the origin of this disease, exclude entirely from consideration, the idea of "all putrifying animal and vegetable substances, all effluvia from jungles and marshes; for every one of the towns," he adds, "which I have visited are, in a remarkable degree, exempted from every thing that could produce these; their situation being either on the rocky bank of a running stream, or on a dry sandy soil."

This may have some foundation; but it is at variance with the statement made both by Mr Gilder, with regard to Ahmedabad in particular, and the province of Guzerat in general, and by Mr Gibson, as to the latter province, which he represents to present all the characters of a highly miasmatic country, abounding in agues, remittents, and bad fevers. This writer, besides remarking the unhealthiness of many individual places, as the Dang, or jungly district between Darrumpore and Soneghur, the Wusravee jungle, the Baroda districts, and the town of Ahmedabad itself, gives the following picture of the state of health in the vicinity of Surat:—

> "The climate between the Taptee and Nerbudda seems to bear a character much the same as to the south of, and about Surat. It has been remarked by many, and I can add my testimony to the fact, that a very bad and typhoid type of remittent fever is met with hereabouts. Hence it is vulgarly called the Kim Chokie, or red tongued fever; the latter name arises from a dry and brick-dust coloured tongue forming a prominent and always present symptom: the type is low remittent with dry skin, and early delirium. It is occasionally, perhaps, rather than commonly met with, which is fortunate, as it is almost always fatal. I have seen the same type of fever only twice in other parts of Guzerat, and these occurred at Kaira, once in a detachment of Golundauze marching from Tunkaria to Deesa in the hot season; the mortality among those affected was great."—P. 58.

The fever here described bears a resemblance not very remote to that which prevailed in Kattywar and Ahmedabad.

These facts incline us to believe that both the province of Kattywar and the Zillah of Ahmedabad present all the conditions requisite for the production of bad agues and malignant remittents; and we think it is so much more likely that the disease may have been of this kind, that marshes, properly so called, are not requisite to the production of malignant remittents; and that the disease appears in a great deal too many places, and affected the inhabitants of each in periods too short to refer its phenomena to the head of plague.

Mr Whyte, on the other hand, rejecting equally the presence and the influence of telluric or miasmatic emanations, proposes to admit as a cause of this distemper, the cause of infectious fever, whatever that may be; and he adduces several facts illustrating the possibility of this mode of origin and propagation. To this it must be answered, that it is doubtful whether typhous fever can exist or spread in a warm climate like that of Kutch and Guzerat. It has been hitherto known as a disease of temperate or cold countries only, and as a disease rather of cold than warm seasons; and it is known that the contagion appears to be dispersed and rendered inert in warm latitudes. It seems, therefore, rather a violent inference to assume that typhous poison could cause this fever.

Amidst this discordance it is not unimportant to remark, that the natives themselves did not allow that the disease was contagious. The brother of the thakoor, of Moolee, we presume, strenuously maintained that the disease was void of contagion; and that contagion was quite a different thing.

Lastly, it must be observed, that even quarantine and restricted intercourse were inadequate to prevent the appearance of the disease at Dundooka in 1820.

In one circumstance we remark a dissimilarity to the plague of the Levant. In none of the accounts is any mention made of carbuncles or even tokens and spots; and had there been carbuncles these surely could not have escaped notice, and even must have attracted observation. No plague epidemic has passed over without carbuncles appearing in a considerable proportion of the patients. This circumstance, we think, shows that there is some difference between the *Ghant ka rog*, the *Kokla ka rog*, and *Tao ka rog*, and carbuncle glandular plague.

It was, properly speaking, an adeno-meningeal or glandular fever; but this gives no information on its nature or causes.

The hemorrhagic form of the distemper, viz. the *Kokla ka rog* and the *Tao ka rog* does not present a very great similitude to oriental plague. Though hemorrhages certainly take place in plague, yet they rarely resemble those which are represented to have taken place in this disease. In plague, hemorrhages take place from the nostrils or bowels, and from the lungs only in those with pulmonary predisposition. In the Kokla ka rog the symptoms were cough, difficult breathing, pain in the breast or side, and discharges of blood from the lungs, till death ensued. In this respect the Indian fever bore a resemblance to the pestilential disease which spread from Italy over Europe in 1348, and which appears to have been a sort of gangrenous pneumonia.

In concluding this account of the Ghant or Indian fever, we think it cannot be doubted that it was the same distemper which in 1836 and 1837 prevailed with such destructive mortality at Pali and the neighbouring towns; and we think that as yet no evidence has been adduced to prove that the disease was plague, and certainly none to prove that it may not have been the product of the country in which it prevailed.

ART. V.—*Outlines of a Course of Lectures on Medical Jurisprudence.* By THOMAS STEWART TRAILL, M.D., F.R.S.E., &c., Regius Professor of Medical Jurisprudence and Medical Police in the University of Edinburgh. Second edition. Edinburgh, 1840. 12mo Pp. 222.

THE science of Medical Jurisprudence is so complicated, and derives its materials from so many different sources, that while it has during the last half century been assiduously cultivated both by English and by foreign writers, it has become so extensive as to require, for the due elucidation of its doctrines, treatises both large and voluminous. This appears quite evident when we compare the compendious treatises of Mahon, Male, Smith, and Metzger, or the first edition of Foderé with the second edition of that laborious and learned writer, the treatise of Paris and Fonblanque, the bulky and elaborate compilation of Beck, or the voluminous work of John Peter Frank. The works first mentioned, though useful and instructive for the time when they appeared, are now no longer capable of giving the student a correct view of the multiplied facts, principles, and doctrines of which the science of medical jurisprudence consists. They belong, indeed, rather to the antiquities and literature of the science than its present state. The treatise of Foderé is an excellent and instructive work, which must, notwithstanding the long time elapsed since its publication, always be perused with advantage, and referred to with confidence. But it is much too voluminous for the student, who would be discouraged by the long though judicious dissertations of Foderé. The work of Beck is also an excellent treatise; but it is not so well calculated for initiating the beginner, as for finishing the advanced student, and, indeed, guiding him in his subsequent study of various branches of the subject. The work of John Peter Frank is altogether out of the question for such a purpose; for, independent of the objection, that it is still confined to the German original and the Italian translation, what courage could encounter, what patience could endure the unheard of labour of wading through nineteen volumes octavo, and listening to the excellent author's endless dissertations on the different modes of educating medical practitioners, and on the best methods of insuring good medical and surgical treatment to the natives of the fourteen circles, or the thirty-nine states of the German empire?

Another circumstance common to medical jurisprudence with several other departments of the complex or applied medical sciences, deserves remark. The changes, improvements, and rectifications from time to time procceding in chemical physiology, in pathology, and in toxicology, are so numerous, that the work which one year gives a complete and correct view of the subject may, in the course of two or three seasons, become defective. Even the excellent treatise of Dr Beck has, in the course of a few years, been twice published in this country, with notes and comments by two able medical jurists; and it has been republished by the author, with all the additional information collected by the author and his brother.

In order to obviate inconveniences of the nature of those now referred to, small compendious works appear to be well adapted. The student, indeed, who is beginning the study of medical jurisprudence, requires in that science, more perhaps than in any other, as an introduction, a short and compendious manual, giving a general view of the principal facts and doctrines of the science. The difficulty is in composing a work of this kind; for the labour of selecting the most important materials and condensing the whole requires, not only intimate knowledge of the subject, but a rare combination of judgment and dexterity.

The present work by Dr Traill we regard as extremely well calculated for the purposes now mentioned. The author does not present it as a treatise, but as outlines of the lectures which he delivers on the subject of medical jurisprudence in the University of Edinburgh. It is indeed a second edition of the outlines published in 1836, extended and considerably enlarged.

Dr Traill divides the science of Medical Jurisprudence into the two general departments of Forensic or Legal Medicine, and Medical Police. The former he distinguishes into three sections; the first devoted to questions affecting the civil or social rights of individuals; the second treating of injuries to property; and the third of injuries against the person.

In the first section, which embraces a considerable multiplicity of subjects, the author treats in a medico-legal view, of the developement of the human frame, the duration of human life, personal identity, marriage, divorce, impotence and sterility, pregnancy, parturition, monsters, paternity and affiliation, survivorship, mental derangement, the rights and responsibilities of the deaf and dumb, diseases exempting from public duties, and simulated diseases.

In the second section the medico-legal circumstances of nuisances, arson, forgery, and counterfeiting the coin of the realm, come under consideration.

The third section is devoted to the medico-legal questions regarding defloration, rape, mutilation, criminal abortion, infanticide, homicide, death by starvation, death from extremes of temperature, wounds, and the extensive subject of toxicology.

The second part, or that devoted to Medical Police, is divided into two sections; the first devoted to the examination of the circumstances affecting the health of individuals, the second to those affecting the health of communities.

In the first are treated cleanliness in all its forms, aliment, the police of apothecaries' shops, clothing, temperance, exercise, prostitution, celibacy and marriage, lactation, and the varied effects of different professions and trades on health.

In the second section the author takes a review of the hygienic conditions and influence of climate, the most eligible sites for towns and habitations, the necessity of drains, and the modes of constructing them, the paving of streets, and the construction and management of public roads, cemeteries, hospitals, schools, prisons, lazarettos and quarantine establishments, and lastly, the subject of punishments.

Of all these subjects the author treats in a clear, judicious, and intelligent manner; and so far as the limits of his work allow, he introduces much interesting and appropriate information. In the selection, indeed, of the important and essential topics from an assemblage so extensive and multifarious as that of medical jurisprudence, Dr Traill has shown much skill and dexterity; and it is fortunate for the student that the preparation of a work so useful, and yet of so moderate pretensions, has been undertaken and accomplished by one so well qualified to do it justice. These outlines may indeed be recommended as presenting an excellent compendious view of the present state of the doctrines in medical jurisprudence; and they will serve as a useful guide not only for initiating beginners, but for conducting the student in his subsequent prosecution of the subject, either generally or in its divisions.

ART. VI.—*The Elements of Materia Medica; comprehending the Natural History, Preparation, Properties, Composition, Effects, and Uses of Medicines.* By JONATHAN PEREIRA, F. R. S., L. S. &c. &c. London, 1840. Part II. Pp. 561—1440.

THE first part of Mr Pereira's work on the Materia Medica, containing remarks on the general principles of classification, and detailed accounts of the inorganic substances used in medicine, was shortly noticed in the fifty-first volume of this Journal.

The present volume treats of the medicinal agents derived from the vegetable and animal kingdoms, arranged according to the natural system, commencing by those lowest in the scale of organization, and advancing to those of more perfect developement. It is unnecessary to remark the great superiority of this mode of arrangement over every other which could have been followed; yet it is a fact not a little strange, that this work is almost the only one in our language in which the natural classification has been adopted, and followed out in all its details. The late works on the same subject have in general failed to bring up the science to the level of the present day; but Mr Pereira's work has fully supplied this defect; and whether the minuteness and accuracy of the details, or the quantity and value of the information be considered, it must be regarded as the most valuable work on the Materia Medica which has yet appeared in this country.

The medicinal agents from the vegetable kingdom are distributed over eighty-one orders; those from the animal kingdom are arranged in ten classes; and the whole is illustrated by woodcuts, representing the general character of the plant or animal from which the medicinal agent is derived. Several of these illustrations might with propriety have been spared, as for instance those of the organs of generation of the Cantharis, when the animal itself is not represented; that of the Opuntia, on which the cochineal insect feeds, whilst the insect itself is omitted; that of a cluster of uterine hydatids illustrative of the action of ergot, and many similar instances.

The volume commences with a tabular view of the history and literature of the Materia Medica, enumerating the various authors who have written on that subject, arranged according to the countries to which they belonged, or whose medicines they described.

The Cryptogamic plants are the first whose properties are enumerated; and it may be proper to mention that, in general, the various species of plants from which medicinal agents are derived are described under the following heads: History,—botanical character,—habitat,—description,—common composition,—chemical characteristics,—physiological effects,—uses,—and form of administration; and the information is both more minute, and embraces a greater number of points than is to be met with in any similar work in our language. Throughout the whole work a high compliment is paid to the editors of the last edition of the Edinburgh Pharmacopœia, their tests for ascertaining the purity of the various drugs being quoted in almost every instance with approbation.

The Carageen or Irish moss of commerce, Mr Pereira limits to one species, the *Chondrus crispus*; but though a large proportion certainly consists of this species, it is not the only one, the *C. mammillosus* being occasionally mixed with it in tolerably large quantity, and possessing in all respects similar properties. Other allied species have also been occasionally met with.

A very full account is given of the Gramineæ.

The growth of ergot is attributed to the presence of a parasitical fungus, whose sporidia become developed on the grain. Its earlier appearance is that of a white coating enveloping the young grain and its appendages, composed of a multitude of sporidia mixed with minute cobweb-like filaments. By the time the grain has extended above the paleæ, it has lost its white coating, and the production of sporidia and filaments has nearly ceased. At the upper portion of the grain, the coating presents a vermiform appearance, and is found, on examination, to consist of masses of sporidia. The mature ergot, however, presents scarcely any traces of filaments and sporidia.

From this opinion, Mr Wright,* one of the latest writers on the natural history and properties of ergot, dissents. He inclines to the opinion which ascribes the formation of the excrescence to the combined influence of atmospheric warmth and moisture. He is the more inclined to this belief, from the circumstance of the ergot being much more common in the district of Sologne than elsewhere, this district being possessed of those properties which favour such a conclusion, viz. moist rich soil, atmospheric warmth, and sheltered situation. Grains also occur, only one-half of which are ergotted, the other half being healthy; an insuperable objection to the opinion, that the ergot is produced by the growth of a fungus, which would equally attack all parts of the grain. Besides, from his own experiments, he found that ergot in powder or substance, sowed with rye, failed to produce the disease on the growing plant; nor did it even succeed when he watered freely and daily the growing plants with water, in which ergot had been steeped. The application of the powder of ergot to the growing ears of rye likewise failed to produce ergotted grains. The excrescences remarked at the upper extremity of the grains of ergot he regarded as the stigmata altered by disease. Mr Wright's conclusions were further confirmed by the fact of his discovery, that ergot contained a considerable quantity of fecula, 26 per cent., a substance which could not have been expected if the disease were produced by a fungus, and which Mr Wright has the merit of discovering, no previous analyst of the substance making mention of it.

---

* Edinburgh Medical and Surgical Journal, Vol. lii. October 1839. p. 293.

Though the Edinburgh College admit but three varieties of aloes, no fewer than seven are described by Mr Pereira, viz. Socotrine, Hepatic, Barbadoes, Cape, Caballine, Mocha, and Indian aloes, each possessing external characters sufficiently distinct. The confusion which has arisen regarding the different kinds of this, and of other drugs occurring in commerce, may, in a great measure, be attributed to the authors of Dispensatories and similar works not taking the trouble to ascertain from the first houses the various kinds which are really brought into the market, and their sources. Mr Pereira's investigations in regard to this deserve all praise, and as instances of the assiduity and labour which he must have bestowed upon this subject, the articles Cinchona barks and Catechus may be referred to.

Five kinds of sarsaparilla are described as occurring in the drug-market, and some excellent remarks are made on the value of this article as a therapeutic agent. The author does not, like many, deny that it possesses therapeutic agency, because no physiological effect has been shown to follow its use.

In mentioning the adulterations to which arrow-root is subjected, it is remarked, that potatoe-starch, or English arrow-root, as it is called, is frequently substituted for it. The only test which he mentions for the detection of this adulteration is that of the microscopical examination of its particles, these being in the genuine arrow-root plane irregular spheres, but in the potatoe-starch having their surfaces marked with concentric rings. A more simple test, and one more generally applicable, is the different external appearance of the two articles, potatoe-starch having a much more glossy or satiny appearance than arrow-root, which is of a duller white. Arrow-root, too, when made so consistent as to turn out in shapes like jelly, retains its form for hours or days, but potatoe starch scarcely retains its shape till cold, subsides considerably, and acquires a tenacious consistence, very widely different from the firm jelly-like consistence of arrow-root.

The different species of Amomum have apparently received considerable attention, and characteristic wood-cuts of each are given,—a circumstance which will make the deciphering of the species to which each variety belongs a much more easy matter than formerly.

A very full account is given of the Coniferæ, and of the various products obtained from them, as well as of the medicinal properties possessed by each of these agents, and the diseases in which they have been or may be employed. This portion of the work is well worthy of a careful perusal, as the numerous diseases in which these medicinal agents have been employed has been lately much extended.

In mentioning the *Jatropha Manihot* or Cassava plant, notice is taken of the singular fact that fermentation or heat destroys the poisonous properties of its juice. It seems not to be generally known that one of the most esteemed and most generally used of the native curries is prepared from the fermented root of the bitter Cassava in Demarara and the neighbouring isles, and eaten almost to every dish.

The narcotic qualities of the nutmeg are alluded to, and several cases quoted as illustrative of the fact. Little attention has hitherto been paid to the narcotic qualities of these fruits, and the few instances on record where such instances have occurred have almost been overlooked. Mr Pereira has not only observed narcotic effects produced by nutmegs, but has found them a useful substitute for opium in diarrhœa. In consequence of their possessing this narcotic property, he cautions the profession against their employment in all cerebral affections.

The question as to the identity of the species of Rheum, which yields the true rhubarb, Mr Pereira leaves still undetermined. His researches, no more than those of others, have been able to solve the question. It is quite possible, however, that our gardens possess the species which actually yields the finest Russian or Chinese rhubarb, the difference of soil and situation causing all that disparity which is observed in the roots produced in the two climates. Six kinds of rhubarb are described as occurring in commerce ; Russian or Turkey, Dutch-trimmed, Chinese, Himalayan, English, and French. He denies that rhubarb exerts any specific influence over the biliary secretion; but the fact, that this medicine, when combined with magnesia and aromatics, is found to be one of the most useful purgative in all bilious affections, and is sufficient of itself to cure the complaint, must be regarded as presumptive proof, at least, that this medicine possesses to a certain degree such a power.

Some useful practical hints are given as to the best mode of employing digitalis; and the precautions to be attended to during its administration. The necessity of keeping strictly to the recumbent position when under its influence is strongly insisted on, as fatal syncope has resulted from suddenly assuming the erect posture. This Mr Pereira attributes to the digitalis so destroying the power of the heart as to prevent its impelling the blood to the brain with sufficient force to resist the power of gravity. He dissents from Dr Withering in what he asserted regarding the diuretic powers of digitalis, viz. that there was but little chance of any diuretic medicine succeeding if digitalis failed. Mr Pereira says he has often seen infusion of broom tops subsequently succeed; and is of opinion, that collections of aqueous fluid are removed, and its specific diuretic action induced by its acting on the kidneys, and not on the absorbents. The quantity of digitalis which may be given to a patient without destroying life is much greater than is generally imagined. One case is mentioned where 20 drops of the tincture were given to an infant labouring under hydrocephalus, three times daily for a fortnight, without any untoward symptom being induced. In a communication from Dr Clutterbuck, it is mentioned that Mr King, a practitioner in Suffolk, had for many years been in the habit of administering the tincture of digitalis in from one-half to one ounce doses, not only with safety, but with the most decided advantage, as a remedy for acute inflammation, frequently, however, premising free blood-letting. His practice was to administer from a half to a whole ounce, and wait the result for twenty-four hours, when, if he did not find the pulse subdued or rendered irregular, he repeated the dose. This, he says, rarely failed to reduce the pulse to the degree wished for, and the disease seldom failed to give way, provided it had not gone the length of producing disorganization of the part. He had administered so much as two drachms to a child nine months old. Vomiting sometimes followed these large doses, but never any dangerous symptom. Dr Clutterbuck mentions that he himself had ventured to give half an ounce dose, but never to repeat it a second time.

The author does not notice the efficacy recently ascribed to lemon-juice as an antidote for an overdose of hyoscyamus.

He merely mentions that a case of poisoning by henbane requires the same treatment as that by opium.

A very full account of nux-vomica and strychnine is given. The author inclines to the belief that this medicine acts on the muscular system through the spinal chord alone; as he found that division of the cord did not prevent poisoning by nux-vomica; whereas destroying the cord caused complete cessation of the tetanic symptoms; or, if only a portion be destroyed, the parts supplied with nerves from this portion of the cord ceased to be affected with convulsions. He also thinks it probable that, as the motor nerves are then chiefly affected, its action is chiefly excited on the anterior columns. The opinion, that the cerebellum is excited by it, he thinks rests on rather hypothetical grounds, though MM. Ollivier, Orfila, and Drogartz have observed in the cerebellum more evidence of lesion than in the other parts of the nervous system after poisoning by this drug. The sexual appetite also, which is regarded as being under the influence of the cerebellum, is generally admitted to be excited by nux-vomica. The indications for the use of this valuable medicine are detailed with great minuteness.

It is difficult to see on what principle Conia deserves a trial as an antidote in poisoning by strychnia. Though the one poison may produce symptoms which are the counterpart of the other, yet, that is no argument for the one possessing antidotal powers with regard to the other. Both are violent poisons, and even were it proved that they were antidotes of each other, no one would be justified in employing a means which of itself would almost surely prove fatal. Mr Pereira applied conia to a wound in a rabbit affected with tetanus from the use of strychnia; the convulsions indeed ceased, but, as might have been expected, the animal died.

A lengthened description is given of the Cinchonas, which are divided by the author into two great classes, true and false cinchonas. The true cinchona barks he divides into two sections; 1*st*, those with a brown epidermis, including three varieties or species, the gray or pale, the yellow, and the red cinchonas; and 2*d*, those with a whitish epidermis, including also three varieties, the gray or pale, the yellow, and the red.

To the pale or gray cinchonas of the first section belong the crown bark, the product of the *Cinchona Condaminea*; the gray or silver bark, that of the *C. micrantha*; the ash bark, that of the *C. ovata*; and rusty bark, the product of the *C. purpurea*. The royal yellow bark, from an unascertained species, is the only bark classed under the yellow cinchonas of the first section. The red cinchona also, from an unascertained species, is classed under the red Cinchonas of the first section, and is the only bark belonging to that variety.

Only one species is classed under the head Pale Cinchonas of the second section, viz. those with a whitish epidermis, the white Loxa bark. Under the yellow Cinchonas of this section are classed the hard Carthagena bark, from the *C. cordifolia;* the fibrous Carthagena bark, probably from the same species; the Cusco Cinchona, from an unascertained species, and the orange Cinchona of Santa Fé, from the *Cinchona lancifolia.* Under the red Cinchona barks of this section are arranged the red Cinchona of Sante Fé, the produce of the *Cinchona magnifolia*, and the red Cinchona with micaceous epidermis of Guibourt.

The false Cinchona barks, only one of which occurs in English commerce, viz. the Pitaya bark, are five in number, viz. the St Lucia bark, the Caribæan bark, the false Peruvian Cinchona bark, the Brazilian Cinchona, and the Pitaya bark.

We do not much approve of the denomination of False Cinchona; a term which indeed originated in confusion and perpetuates error. It seems rather absurd to speak of False Cinchona, since the use of the term implies that the barks so designated do not belong to that genus. It is preferable to designate the barks so meant by their correct name of *Exostemma*, to which all those now mentioned are known to belong, and by the allied term of *Cosmibuena*, when they are referable to that head.

Regarding the alkaloids of the barks, Mr Pereira justly remarks, that he cannot subscribe to the opinion, that they possess all the medicinal properties of the barks themselves, seeing they are deficient in aromatic qualities, and possess no astringent properties. It is to this circumstance that he attributes the fact, that disulphate of quinine will sometimes irritate the stomach, and give rise to nausea and pain, whilst the infusion of bark will be retained without the least uneasiness.

The process of the Edinburgh College for the preparation of the disulphate of quinia is highly commended, combining as it does both simplicity and economy.

Under the heads of *Areca Catechu*, *Uncaria*, *Gambir*, and *Acacia Catechu*, are given the most satisfactory and full accounts of the numerous varieties of catechu yet published. The species of Cassia yielding senna are well characterized, and beautiful representations of the leaves are given, as well as of those which are used in the adulteration of that drug.

With regard to Quassia, Mr Pereira thinks that it probably possesses somewhat deleterious properties; and though he is not inclined to place much confidence in the reports of Kurtz, Barbier, and Kraus, as to the poisonous properties of quassia, especially as to its action on the nervous system, he thinks that a cautious practitioner would avoid employing it in amaurosis and cerebral affections. We have frequently witnessed unpleasant effects caused by the administration of the infusion of quassia. The chief symptoms produced were, sickness, prostration of strength, and general tremors; and one individual was so easily affected by this medicine, that he could detect its presence in any bitter infusion, in however small quantity it might be, from its effects on his system.

A most valuable and comprehensive article is given on opium. Eight varieties occurring in commerce are described, with the external and chemical peculiarities of each. The ingredients found in opium are minutely described, with their modes of preparation and their tests. In mentioning the various processes which have been suggested for the ascertaining the proportion of morphia, he takes occasion to condemn that recommended by the Edinburgh Pharmacopœia, as the morphia is soluble in a solution of carbonate of soda. There must be some misunderstanding, however, as to this mode of conducting the experiment, since, if the precipitation be conducted in the cold, as directed, the whole morphia is precipitated. If heat be applied, however, to favour the precipitation, as seems to have been done by Mr Pereira, a considerable quantity of the precipitate is redissolved. We have frequently repeated the process, and found it to answer sufficiently. It has, besides, the great advantage over others of being easily applied, the ingredients required being always at hand. But the author prefers a modification of M. Thiboumary's process, though complicated, as the best means for determining the goodness of opium by ascertaining the quantity of morphia.

In mentioning the various forms in which cantharides are employed as a vesicant, we wish that Mr Pereira had noticed one preparation which is much lauded by the French, and which, within the last two years, has been pretty generally introduced into practice in London and Edinburgh. The preparation alluded to is the etherial extract of cantharides, spread over gummed cambric-paper. It possesses some advantages over the cantharides blister in common use. It is less apt to affect the urinary organs, though it does not act more speedily, and not always so surely; it is more cleanly, and may be carried fit for use in a common pocket-book. That prepared in London is spoiled by the addition of some balsamic tincture, to stain the paper of a yellow colour, and render its odour more pharmaceutical. Messrs Smith in Edinburgh prepare it in the same form as that originally used in Paris.

ART. VII.—1. *Report on the result of the operations for the Cure of Sguinting, performed at the Royal Westminster Ophthalmic Hospital, between the 18th April and 30th October* 1840. Presented to the Governors at their Quarterly Meeting, 5th November 1840. By CHARLES W. G. GUTHRIE, Junior, Assistant Surgeon to the Hospital, &c. Westminster, 1840. Pp. 12.

2. *A Practical Treatise on the Cure of Strabismus, or Squint, by Operation and by milder Treatment; with some new Views of the Anatomy and Physiology of the Muscles of the Human Eye.* By P. BENNETT LUCAS, M. R. C. S. L. &c. Illustrated by Plates. London, 1840. 8vo. Pp. 91.

IN our Fifty-Fourth Volume (July 1840, p. 259,) we gave some account of an operation which had been successfully performed at that time in three cases for the cure of squinting, by Dieffenbach of Berlin; and stated that the operation had been already introduced in this country by various surgeons in London and Edinburgh. It has indeed now become an exceedingly common operation, having been performed already in several hundreds of cases. Mr Charles Guthrie, who is one of the greatest labourers in this field, alone, has performed 340 operations for internal or converging squinting, and 16 for external or diverging squinting.

His report is drawn up with great ability, and shows perfect knowledge of the cases best adapted for operation,—a very correct estimate of the value of the operation itself,—and much information on all those circumstances which tend to render it abortive or to limit the benefits which it confers.

Mr Bennett Lucas, who has also been a diligent labourer in the same field, has given in the work before us the most complete account hitherto published of the pathological theory of squinting, and its different varieties and causes, and of the circumstances requiring or contraindicating the performance of the operation.

After a description of the muscles of the eye-ball, Mr Lucas gives the following account of the movements of both eyes, and of the part performed by the motific nerves.

“ When the movements of both eyes are made the subject of investigation, they do not admit of the same satisfactory method of demonstration as the movements of one of these organs, and the harmonious actions which they observe, call for another method of investigation, besides that of making traction upon their fibres in the dead subject. In writing at the present moment, my eyes are directed downwards and inwards; I can relieve myself from the occupation in which I am engaged, and turn both eyes upwards, or direct them downwards, and, when I resume the writing posture, they are again directed downwards and inwards.

" By no exertion can I direct both eyes outwards, or both downwards and outwards. In short, the ability to turn both eyes outwards is altogether forbidden; even in the new-born infant, when the eyes, by their irregular movements, evince the difficulty it experiences in its endeavours to fix them on an object, they never take either of these directions. And when we reflect that the direction of the eyes inwards is that which is continually being called upon in the exercise of our varied avocations, we cannot but be struck with the care which has been taken to accomplish this end.

" Were both eyes capable of being directed outwards at the same moment, we would, it is true, be thus enabled to take an extensive field of vision on either side laterally and somewhat posteriorly, but then we would altogether lose sight of objects placed directly before us, and would be in a similar condition to those lower animals in which the eyes are so much behind, that they cannot see objects directly placed before them, and often suffer in consequence. The actions, then, of the two external recti muscles, and of the two superior oblique muscles, are remarkably contrasted with the actions of the other pairs. When either eye is drawn outwards by the action of the external rectus muscle, the other is directed inwards by the action of the inner rectus and the inferior oblique muscles; or when one eye is directed downwards and somewhat outwards by the action of the superior oblique muscle, the other is directed in the same proportion downwards and inwards in the diagonal of the inner and inferior recti muscles, so that in no movements of the eyes are objects placed before us lost sight of, for to the same extent that one eye is directed outwards the other is directed inwards."—Pp. 14, 15.

As he infers that these opposite motions enjoyed by two pairs of ocular muscles, and not by the others, must depend on some peculiarity in the nerves supplying these muscles, he observes, that while the muscles, which act together in moving both eyes in the same direction, are supplied by the third pair, or oculo-muscular nerves, those which do not present this consentaneous action, viz. the external recti and superior oblique, are supplied by distinct nerves; an arrangement which, he thinks, gives these latter muscles greater energy of action to compensate for the unfavourable position which they hold, to the organ they are to move. He then proceeds:—

" Other peculiarities, besides those I have noticed, distinguish the motor nerves of the eye. All of them are connected with the sympathetic nervous system by numerous filaments, particularly the abducens nerve, which receives two large ones, and which are in close contact with the internal carotid artery. These connections of the motor nerves of the eye with the sympathetic are so well known that it is unnecessary to dwell upon them here, further than to notice the fact, as it will serve to throw light upon those instances of temporary strabismus. which accompany affections of the *primæ viæ*, and of the respiratory organs.

" The third nerve, or that which supplies four of the six muscles, is further distinguished from the others, in consequence of one of its branches, that which goes to the inferior oblique muscle, forming a remarkable communication with the small body, called the lenticular ganglion, from which the nerves for the supply of the iris proceed.

" The contiguity which the motor nerves of the eye hold to several of the large blood-vessels of the brain is most remarkable, and will serve to account for many of those interesting cases of strabismus which follow determinations of blood to this organ.

" The third or common motor nerve of the ocular muscles, at its connection with the brain, is situated between two large and important arterial trunks, viz. the posterior cerebral and the superior cerebellar arteries, and as it passes forwards and outwards, it also holds a close relation to the cavernous sinus, being situated in a fibrous canal in its outer wall. After these relations to blood-vessels it arrives at the foramen lacerum, before passing through which, it divides into two branches; the smallest of which supplies the superior rectus, and the levator palpebræ muscles, and the largest, the inner rectus, the inferior rectus, and the inferior oblique muscles. The branch which supplies the inferior oblique sends a filament to the lenticular ganglion.

" The fourth nerve, although the most remarkable of all for its long and tortuous course, its delicacy, its forming communications with no other nerve, its being distributed to but one muscle of the eye, and its presenting the same peculiarities in all animals in which it is present, is not so intimately connected with the cerebral vessels as the others. From its origin it passes forwards, and before reaching the foramen lacerum, to arrive at its destination, it enters the outer wall of the cavernous sinus.

" The sixth, or abducens nerve, from its origin at the upper margin of the corpus pyramidale, passes forwards and outwards, pierces the dura mater near the attachment of this membrane to the posterior clinoid process, and enters the fibrous wall of the cavernous sinus; it passes through the foramen lacerum, and runs between the origins of the external rectus muscle to the ocular aspect of which it is ultimately distributed by numerous filaments. This nerve, at its connection with the corpus pyrimidale, is in relation with the vertebral artery of the corresponding side; afterwards it is situated external to the basilar artery, some of the branches of which cross it, and here the nerve is placed between the pons Varolii and the basilar process of the occipital bone. The next blood-vessels it holds intimate relation to, are the cavernous sinus and the internal carotid artery, being separated from the blood contained within the former merely by the lining venous membrane, and being closely situated at the external side of the latter. At its point of crossing the carotid artery two very remarkable filaments connect it with the superior cervical ganglion of the sympathetic nerve.

"Besides the connection which we have seen to exist between the nerves which go to the muscles, the sympathetic nerve, and many of the great cerebral vessels, in consequence of which the actions of the muscles of the eye will more or less sympathise with derangement of the circulation within the head, or with derangement of those organs over the function of which the sympathetic nerve presides; these active organs of locomotion are moreover influenced by derangements of the passive organ, the eye, which they are destined to move. Thus, in opacities of the cornea, the eye will be often found to squint for the purpose of admitting the rays of light through its transparent portion, and in many instances I have seen patients who had lost the sight of one eye, from various causes and at various ages, become afterwards affected with permanent strabismus. It, however, not unfrequently happens that the weakness of vision is consecutive upon the strabismus, and evidently arises from the patient not making as much use of the eye which squints as the sound one. Where the blindness precedes the strabismus, little hope can be held out to the patient of improving the sight by the operation of dividing the muscle which produces the squint, although, by performing such, the appearance of the countenance will be remarkably improved; a fact which has induced young persons, especially females, to submit themselves to the operation; and in all instances it has been attended with the anticipated results. But in those cases where the weakness of sight is consecutive upon the strabismus, not only has the eye been relieved from its deformity by operation, but its sight has in many cases progressively and steadily amended."—Pp. 15–18.

In speaking of the insertions of the muscles of the eye, he finds that the general representation, that the recti are inserted at equal distances from the circumference of the cornea, is not well founded; and he rectifies the descriptions from repeated dissection in the following manner:

"All the muscles of the eye are inserted by flat, shining parallel tendinous fibres into the sclerotica, and this membrane, which forms the case of the eye, being globular, the shape of the insertion of each muscle necessarily forms a portion of the segment of a circle, the convexity towards the cornea; the consequence of which is, that the centre of the insertion of each tendon is nearer the cornea than its ends—a fact which would be likely to make a careless operator suppose that he had divided all the tendon of a muscle he was operating upon, when he had only divided its central fibres, leaving the extremities still attached.

"The tendons of the superior and the inferior recti muscles are of equal breadth at their insertions; the breadth of the inferior oblique exceeds that of the superior by about a line, and the breadth of the internal rectus exceeds the breadth of the external by nearly one-third. These measurements are pretty constant in all subjects.

"In the well-proportioned eye of an adult, the centre of the tendon of the superior rectus, and that of the inferior rectus, are at an equal distance from the cornea, viz. four lines; the inner edges of each are about the same distance, whilst the outer edges are considerably more removed, being distant seven lines.

"In consequence of this arrangement, both muscles will fairly antagonise each other in their alternate actions of raising the eye upwards and downwards; but, in consequence of the close proximity of the internal fibres of their tendons to the cornea, in comparison with the distance of their external fibres from this point, the former have much greater power in directing the eye inwards than the latter have in directing it outwards.

"The centre of the tendon of the internal rectus muscle is distant from the cornea about three lines, its superior edge is distant four lines, and its inferior edge five lines.

"Contrasted with the tendon of this muscle, that of the external rectus is distant at its centre from the cornea nearly five lines, and its superior and inferior edges are distant nearly six lines.

"From this arrangement it is evident, that the internal rectus muscle has more power in directing the eye inwards than its antagonist, the external rectus, has in directing it outwards: and from the superior fibres of the inner rectus advancing more towards the cornea, they have more power in giving the eye a direction upwards, than the inferior fibres have in giving it a direction downwards.

"The centre of the tendon of the superior oblique is distant eleven lines from the cornea, and that of the inferior oblique is distant fifteen lines from the same point, so that operation upon these muscles at their insertion is out of the question. The origin of the inferior oblique at the depression of the lower margin of the base of the orbit, and the pully of the tendon of the superior oblique at the upper and inner angle of the base of this cavity, are the two situations to select for the section of these muscles.

"In four eyes, the transverse axis of whose orbits were nineteen lines and a-half, the situation of the pully and tendon of the superior oblique muscle was four lines and a-half distant from the inner extremity, and was five lines deep, measuring from the upper edge of the orbit at this point; and in the same eyes the origin of the inferior oblique was four lines and a-half distant from the inner extremity, and two lines deep, or behind the margin of the base.

"I have in one instance divided the superior oblique muscle in the living subject, without producing any perceptible effect upon the motions of the eye-ball. The case was one of convergent strabismus, in which I had previously divided the inner rectus muscle, and also removed the subconjunctival and submuscular fasciæ from the inner surface of the eye-ball without producing that complete satisfactory result which attends this operation. By prolonging the incision of the conjunctiva upwards, I succeeded in passing the blunt hook around the tendon of the superior oblique muscle, and not merely divided, but excised a small portion of it. The peculiar rounded form of the portion of the tendon which was removed, made it evident to all present that it had been fairly cut through.

"In the dead subject, the pully and tendon of the superior oblique muscle, and the origin of the inferior oblique muscle, can be readily got at by making a semicircular incision of the integuments corresponding to their respective positions. In the former operation the inner division of the frontal nerve should be avoided. If it be attempted to get at either of these muscles by making an incision of the conjunctiva, the operation will be found to be more difficult."—Pp. 19–22.

A good description of the conjunctiva follows; but we proceed to the author's account of the varieties and causes of strabismus or squinting.

"*Causes of Squint.*—Squint essentially depends upon the perverted action of one or more of the muscles, which are provided for the movements of the visual organs; it therefore follows that this affection may not only assume various degrees of intensity, but also various directions, according to the muscle or muscles which are at fault.

"From the anatomical and physiological remarks which have preceded this treatise, it is apparent that the muscles of the human eyes may become perverted in their action from various causes.

"As muscles they are liable, like similar organs in other parts of the body, to lesions of their nutrition, by which they become wasted or atrophied; may become more than naturally developed, or hypertrophied; or may become altogether changed in their structure.

"The intimate connection which exists between the motor nerves of the eyes and the sympathetic nervous system, will account for those numerous cases of temporary strabismus, which depend upon affections of the digestive organs; and the remarkable connections of these nerves with the brain, and their close relation to many of the cerebral blood-vessels, will also as satisfactorily explain those inveterate cases of permanent squint which so often follow diseases of the brain, and determinations of blood to that organ.

"Not only are we thus furnished with fruitful causes of strabismus in most of those affections incidental to early life, and in many which occur in riper years, but in consequence of the muscles of the eyes being subservient to the optical instruments which they are destined to move, diseases of the latter, and even of their appendages, are also frequent causes of squint.

"External violence to distant parts and to the parts in the immediate vicinity of the orbits must be also enumerated as causes of strabismus, and also habit or imitation."—Pp. 26–27.

Hypertrophy or increased developement of the inner rectus muscle, the author has had many opportunities of observing to cause convergent strabismus. The muscle is then not only increased in size, but unusually vascular and of a deeper red colour than natural. When such an inner rectus muscle is divided, it effuses more blood than usual, and it is connected by strong adhesions to the sclerotic coat, and at the end of ten or twelve days gives the organ an inclination inwards greater or less, but never to the extent it had previous to the operation.

On the subject of strabismus from atrophy, the following observations are made:

" The opposite state to the condition of muscle I have just noticed, as a cause of strabismus, is rather to be inferred than demonstrated; for, if any of the muscles of the eye be in a state of atrophy, their antagonists, without any unnatural degree of developement, will necessarily act to an advantage incompatible with the healthy movements of the eye, and produce strabismus, to cure which we operate upon the muscle which is not virtually at fault, but upon that whose increased action is owing to the weakness of its antagonist, and not to any power residing in itself from an increased developement of its fibres.

" In our operations, therefore, for the cure of convergent strabismus, when the internal rectus muscle presents no deviations from a state of health, although we have no means of actually ascertaining the condition of the external rectus muscle, it is a reasonable inference to draw that it is more or less wasted. It cannot be paralyzed, otherwise it would be incapable of drawing the eye outwards, which in all the cases of convergent strabismus I have operated upon, it had more or less the power of doing.

" In one case of divergent strabismus, when the external rectus muscle was exposed, it was remarkably wasted, and, notwithstanding, it had the power of producing divergent squint. When the muscle in this case was divided, the eye was not drawn forcibly inwards, as it otherwise would have been, had the integrity of the inner rectus muscle been perfect. The eye was capable of being drawn as far as the inner canthus, but the patient could not turn it deeper, as can be effected to a very considerable degree when the inner rectus muscle is healthy. The inner rectus muscle must have suffered some change, so as to completely interfere with its function, and in consequence of which the outer rectus muscle, notwithstanding that it was so much atrophied, had the power of producing divergent strabismus.

" It is not uncommon to find the internal rectus muscle to present marked deviations from its natural structure. This is not surprising, when we consider the changes of structure which take place in muscles elsewhere; but whether these deviations are subsequent or antecedent to the strabismus, is not easy to determine."—Pp. 30—32.

Strabismus is sometimes the result of disorders of the alimentary canal reflected upon the brain or its membranes, or upon the abducent nerve by means of the great sympathetic, and, it may be, the result of various affections of the brain or its membranes.

"Sudden determinations of blood to the brain are fruitful causes of strabismus. My notes of the cases in which I have operated show that, in numerous instances, strabismus followed a paroxysm of hooping-cough, a convulsion and attack of croup, &c., and in the adult I have traced two well-marked cases of convergent strabismus to excess in venereal indulgences.

"A most interesting case of strabismus, produced by this cause, was lately communicated to me by an eminent physician. The patient was young, and was suddenly attacked with strabismus, which, after a few weeks, was completely cured by the detraction of blood and other antiphlogistic measures, accompanied with the utmost caution in his manner of living. Some months afterwards he indulged as freely as before, and died from cerebral apoplexy.

In all those cases the squint took place suddenly, and resisted various kinds of treatment. The relation of the nerves which supply the ocular muscles, particularly of the third and sixth to the blood-vessels of the brain, will serve to throw some light in accounting for those determined cases of squint which arise from an undue quantity of blood being suddenly sent to that organ; and, in the two cases I have mentioned, if the relation of these nerves to the arteries which supply the cerebellum and adjacent cerebral masses be considered, a still more satisfactory explanation of strabismus following excess in venereal indulgences offers itself.

"Temporary forms of strabismus are also caused by mental emotions, and I have in many cases seen that permanent squint was always aggravated by anxiety and irritation."—Pp. 36–37.

The forms of strabismus, Mr Lucas distinguishes into, 1. convergent; 2. divergent; and, 3. anomalous; and the convergent he distinguishes into single convergent, or that affecting one eye, and double convergent strabismus, or that affecting both eyes.

The two varieties of convergent strabismus are described in the following manner:

"Convergent strabismus may affect one or both eyes; when both eyes are affected, they never are turned in at the same time, so as to hide portions of both corneæ, although when one cornea is more than half hid, the other has generally a slight inclination inwards. Sometimes, however, one eye will remain in the centre of the orbit when the other is turned upwards and inwards.

"A patient with double strabismus may lead an inattentive observer to think that one eye only was affected, did he merely depend upon his taking a cursory glance at the countenance. I have met patients with double strabismus, who would for several hours use but one eye in viewing objects, whilst the other remained buried in the inner canthus, but the moment the hand was placed over the apparently straight eye, the organs, as it were, changed places, and that which was before straight became inverted, and the other occupied a natural position in its orbit.

" It is of importance, then, in all cases of convergent strabismus, to examine carefully the condition of both eyes before operating, lest, when one eye was restored to a natural position, the other would become inverted, and although this is actually what occurs in such cases when the inner rectus muscle of one eye in double convergent strabismus is divided, yet, if the surgeon was not aware of the fact, and did not prepare both the patients and their friends for the consequence, on the eye opposite to that operated upon, he would get the credit of substituting one squint for the other.

" To ascertain the state of both eyes, and the actual amount of deformity which exists, I have found the following method most satisfactory. If it is the right eye which is inverted, and the patient is employing the left for vision, I place my hand obliquely over the left eye in such a manner as to hide all objects in front of it, but keep the hand sufficiently raised at the temporal margin of the orbit to enable me to watch its movements. I then desire the patient to exercise the eye which is uncovered; and if at the time that he brings it to the centre of the orbit, the covered one retreats into the inner canthus, the case is one of double convergent strabismus, and both eyes will require to be operated upon; but if the contrary occurs, if both eyes are at this period straight, or even if the covered eye has but a slight inclination inwards, the case is one of single convergent strabismus, and the inner rectus muscle of one eye only will require to be divided. I have in several instances seen the eye which was not operated upon, have a slight inclination inwards for many days after the operation; but, when the healing process was completed, and both eyes were freely exercised by the patient, this inclination inwards gradually became less and less."—Pp. 48, 49.

" Convergent strabismus of one eye is that which is most frequently met with; it varies in degree in some being very slight, and in others the eye is so much turned in as to interfere altogether with its functions, unless the unaffected eye be closed.

" In convergent strabismus of one eye, after the inner rectus muscle is divided, the organ in some cases, not only becomes straight; but also prominent, so as strikingly to contrast with its fellow.

" This has occurred in patients in whom the eyes were naturally full and prominent, as well as in those in whom the eyes were small and sunken; but in the former cases, after a fortnight or three weeks interval, both eyes appeared equally well set, whereas in the latter one eye remained prominent, and the other sunken, although that both eyes were perfectly straight.

" In these latter cases, if the inner rectus muscle of the sunken eye be divided, the operation will be attended with the best effects; both eyes will be equally prominent, and, contrary to what might be expected, they will also be equally straight, no trace of strabismus remaining."—Pp. 51, 52.

Divergent strabismus is easily understood from its name. It is that in which the affected eye takes the direction opposite to that which has just been considered. It may be well to illustrate this form of the disease with the following case, in which the effects of operation are also mentioned.

" August 1st, 1840.—Jane Johnson, *etat.* 30, has had divergent strabismus of the left eye since she was eight years of age. Her eyes are prominent, and the irides are of a grey colour. When both her eyes are open, and the right one is directed forwards, the left occupies the external canthus; when she turns the right eye as much outwards as possible, the left then occupies a position a little internal to the axis of the orbit; when the right eye is covered, so as to exclude the light, she can then turn the cornea of the left eye as far as the plica semilunaris, but she is incapable of turning it deeper so as to hide any portion of the cornea—a motion which can always be effected in the natural condition of these organs.

" In the presence of Dr Léon Husson, Dr Dolce, Mr Cameron, and Mr Sharpe, I divided the tendon of the external rectus muscle.

" The following is an exact account of the movements of both eyes after the operation:—

" As soon as the division of the tendon of the external rectus muscle was completed, the eye occupied the axis of the orbit; when the patient moved the sound eye to its fullest extent outwards, the eye operated upon occupied the inner canthus, but did not turn so deep as it does when both eyes are unaffected; in this respect, the same phenomena presented themselves as when the inner rectus muscle is divided for convergent strabismus of one eye.

" The strabismus in this case must have been owing to weakness of the internal rectus muscle, or, perhaps, total or partial paralysis of it; otherwise there was nothing to prevent the eye from being turned deeply inwards, so as partially to conceal the cornea, and the oblique muscles were capable of producing the inward movement which took place.

" August 16th.—There is no appearance of strabismus. A small granulation has sprung up in the incision of the conjunctiva, which I removed with a hook and flat pair of scissors."—Pp. 53, 54.

Squinting, it must be manifest, proceeds from different causes; and in all cases of the deformity the same remedy is not admissible. There is some danger, we think, lest, in the present fancy for operating for the cure of squinting, the operation may be employed where it can be of no service. Of this evil Mr Lucas is perfectly aware; and we think the following remarks on the treatment very judicious.

" The new operation, it should be borne in mind, is not applicable to every form of strabismus; innumerable cases present themselves where it is inadmissible, or which can be cured by milder measures; and the surgeon, who operates without first investigating the history of the case, and accurately ascertaining the amount of deformity which exists, is guilty towards his patient, on the one hand, of inexcusable rashness, and towards his profession, on the other, of bringing a safe and efficacious operation into unmerited disrepute.

"The novelty of an operation, together with its simplicity and safety, too frequently induce a thoughtless application of it, regardless of the after consequences.

Two cases presented themselves to me some months ago, to be operated upon for convergent strabismus. On examining the condition of the eyes, one cornea in each was found to be densely opaque; in one case for half its circumference, in the other for a smaller extent. Both squints were consecutive upon the opacities, and evidently took place for the purpose of allowing the rays of light to pass through the transparent portions of the cornea.

"Mr Pettigrew, Dr Hennis Green, Dr W. Pettigrew, Mr Cameron, and others who were present, agreed that it would be rash to interfere with this wise provision of nature, to secure vision to the organ by operation, the effect of which would be to straighten the eye, and counteract her obvious intentions.

"One of these patients has since been operated upon elsewhere, and I had an opportunity some days since of seeing his eye straight and useless!

"That many cases of strabismus which depend upon atrophy or hypertrophy of a muscle, can be cured by other means besides operation, the records of surgery afford abundant evidence. Within the last six months I have met adults who, when children, were the subjects of strabismus, and who wore various mechanical contrivances by which they were cured; and I have met others who were subjected to a similar treatment in which the strabismus was very much corrected.

"In cases of strabismus, where no previous disease or accident occurred to which the affection could be attributed, and where there is no disparity in the vision of both eyes, it may be inferred that the cause of the deformity exists in the muscles themselves: and such cases require the most serious reflection regarding the propriety of an operation, or the adoption of other treatment. In children this deliberation is especially called for. The muscles of the eye, in common with all other parts of the body, are at this time being developed, and by frequently calling into action the weaker muscle by a well regulated amount of exercise, there is every reason to expect, and experience proves it, that after a time it will acquire its proper magnitude and healthy amount of power.

"There are no organs in the body whose size and power can be increased and regulated in a greater degree, by well directed exercise, than the muscles. The history of gymnastics, the correction of deformities by bringing muscular action into play, as in crooked spine; and the well-known treatment of Hunter in the case of the patient, the portions of whose fractured patella united at a distance; are a few of the many instances which might be brought forward.

"If a muscle of the eye be altered in its structure, such as we find the sterno-cleidomastoid to be in wry neck, any amount of power which the opponent muscles may obtain will have little effect in correcting the strabismus, for in this case the muscle producing the strabismus will be to the eye what inelastic ligament is to the articulations, and an operation for the section of the altered muscle becomes indispensable.

"The history of the case, and the power which the patient possesses of turning the affected eye from its unnatural position, will enable the surgeon to form a pretty accurate diagnosis of the condition of the muscle which produces the deformity. Where the patient was capable, under any circumstances, of turning the affected eye external to the axis of the orbit, and especially when he had the power of keeping it in this position for a short time, both eyes being open, I never found any alteration in the structure of the muscle upon dividing it. In some such cases, it was more developed than natural, but in others it appeared of its natural size, and I have occasionally even found it pale and very thin. But where the patient had not the power of turning his eye more than a few lines from the inner canthus, and could not retain it for a short moment, even so much outwards, both eyes being open, I as invariably found the muscles altered in its structure, or most unnaturally developed."—Pp. 55–58.

"I have had already occasion to mention some cases of temporary strabismus in children dependent upon derangements of the primæ viæ, which have steadily improved as their bowels were brought to act healthily.

"Such cases will require constant watching, lest, from repeated attacks of temporary strabismus, the disease may at last assume the permanent form, and require less manageable remedies for its cure, or ultimately operation.

"Strabismus, depending upon the presence of worms in the alimentary canal, is to be promptly treated by the removal of the cause which induces it; and where it depends upon dentition, the treatment is equally plain.

"Some of the most serious forms of strabismus attend diseases of the brain, and determinations of blood to that organ; but here, unfortunately, the treatment of the strabismus is of very minor importance compared with the treatment of the cause which induces it.

"In hydrocephalus strabismus is a common occurrence.

"In inflammation of the brain, strabismus is so constantly met with, that systematic writers usually describe its forms, when treating of this affection, and attribute its presence to compression, occasioning more or less paralysis of the motor nerves of the eye.

"It is only in cases where the patient recovers from any of these diseases with strabismus, that operation upon the muscles of the eye or other treatment, becomes necessary; and the judgment of the practitioner must be guided by the condition of the muscles of the affected eye, their relative amount of power, the intensity of the strabismus, and the length of time which it has existed.

" When strabismus in the adult follows determinations of blood to the brain from violent mental emotion, excess in venereal indulgences, the abuse of spirituous liquors, and other such exciting causes, the affection is under the control of milder remedies than operation, provided these measures be speedily put into practice, and steadily persevered in.

" I have alluded to cases in which the best success followed strict antiphlogistic measures. Such treatment is so familiar, and is so fully detailed in various writings, that it is unnecessary to dwell upon it here; and it also need scarcely be mentioned, that a cautious abstinence from the cause which produced the determination of blood to the brain should be strictly adhered to. I have always observed that where strabismus arose from sudden determinations of blood to the brain, such as follow a paroxysm of hooping-cough, croup, &c. it took place almost instantaneously.

" Where strabismus is consecutive on measles, small-pox, and the other exanthemata, there is every reason to attribute it to the effect which these diseases produce upon the conjunctiva, in common with the skin and mucous membranes of the body generally.

" Strabismus, in like manner, follows repeated attacks of conjunctivitis.

" In such cases, the practitioner would be much to blame if he proceeded at once to divide the tendons of any of the ocular muscles, until he tried other remedies.

" As the affection of the conjunctiva subsides under the use of collyria and constitutional treatment, the squint will also in most instances disappear. If it should not, there need be no hurry to operate, for the eye can be straightened at any time by this measure.

" In those cases, where strabismus arises from imitation, habit, unusual position of the head, and such causes, its treatment resolves itself into removal of the cause which induced the patient to acquire the squint; and the healthy exercise of the muscles in the opposite direction, either by the voluntary power of the patient or by mechanical contrivances.

" In like manner, the early removal of tumours, nævi, &c., when squint is dependent upon their presence, will prove equally successful; but if these have existed any length of time, the eye, after their removal, will still be frequently directed to the situation they occupied, particularly if the patient be excited.

" Strabismus, arising from injuries of the head, are sometimes temporary, and disappear with the other symptoms; it constantly happens, however, that it remains after the other effects of the injury have subsided, and resists all treatment except by operation.

The worst forms of strabismus of one eye, and of both, have followed such accidents.

"The affections of the eye itself, which are followed by strabismus, are very numerous, and it requires a cautious examination of the history of the case and the conditions of both organs, before an operation be resorted to.

"Where squint arises from nebulous cornea, in many instances it will be benefited or entirely cured by remedies calculated to remove the nebula; such as the administration of mercury, and the use of stimulating collyria. But where the strabismus is dependent upon dense opacities of the cornea, such as follow deep ulcers of it, the less either the strabismus or the opacity is interfered with, the better."—Pp. 60–62.

The method of operating is described in the following manner:

"The patient, then, being properly adjusted, and, if necessary, secured, the position which the assistants are to hold to him, and the operator is next to be considered. In general, where the patient is not unruly, two assistants will answer all purposes—or, indeed, one will suffice, as the principal use of the second is to hand the instruments when required, a proceeding which may be dispensed with, if the operator arranges his instruments before-hand on a small table within his reach. The principal assistant stands behind the patient, and should be sufficiently raised above him to have full command over his head and eye-lids. The head of the patient should be allowed to rest against the lower part of the assistant's chest, and at the same time that he is securing the eye-lids, he is thus also enabled to steady the patient's head.

"To effect the former purpose, the best instruments are the fingers of the assistant. I am aware that several inventions have been contrived for the same purpose, such as the wire speculum, the solid speculum, the blunt retractors, and so on; and of late that a forceps has been used which is made to grasp the conjunctiva, and is allowed to hang from this delicate membrane, with the professed view of depressing the lower eye-lid by its weight.

"A more clumsy or a more unnecessary proceeding than this could not have been devised; unnecessary, because the finger, as every one knows, is sufficient to depress the lower lid for all practical purposes better than any instrument; and clumsy, on account of the continued drag which a heavy forceps must make upon the tunica conjunctiva, and its being apt to let go its hold during the operation.

"By using the fingers in securing the eye-lids a double object is attained. In the first place, the natural softness and temperature of the fingers, when applied to the delicate tegumentary covering of the eyelids, never produces that degree of irritability in their orbicularis muscle, which contrivances of human invention in general do; and, in the second place, when these natural retractors are judiciously applied, they are always sufficient to keep the lids apart, to regulate the degree of separation required, and to allow them to close when it is desirable that they should do so, better than a speculum, a forceps, or any other such clumsy inventions for this purpose.

"Supposing that it is the right eye which is the subject of operation, the assistant who supports the patient's head should place the fore-finger of his right hand on the integuments covering the malar bone near its junction with the superior maxillary, a few lines beneath the lid, and should draw them downwards and a little outwards, not with a great degree of force, as is sometimes done, but gently : if too much force be used, the conjunctiva, as it passes off the globe of the eye to the eye-lid, and the tarsal cartilage become everted and swollen, and more or less conceal the globe.

"The fore-finger of the left hand should be employed in elevating the upper lid, the integuments of which should be gently raised towards the supra-orbital ridge, and there steadily compressed against it. This should be performed three or four times before commencing the operation, as it accustoms the patient to it, and lessens the chance of his offering unintentional opposition by the contraction of the orbicularis muscle. The operator also, before commencing the section of the conjunctiva, will find it advantageous to touch this membrane, where it covers the inner rectus muscle, three or four times with the handle of his hook, or the point of his finger with the same object.

"These observations may appear to some of my readers, of but trifling importance, but it should be remembered that I am not treating of an operation which involves one-sixth of the human body, as amputation at the hip-joint, or the removal of the facial bones by a chisel and mallet, or forceps ; where, in either case, the resistance offered by the sufferer to the surgeon is not of much moment as regards the immediate object of the operation, and can be overcome by physical force ; but that I am describing an operation which has for its object the division of the tendon of a muscle which is so allied at its insertion to the sclerotic coat of the eye, that it may be said to form a part and parcel of that membrane, and a wound of which may be followed by serious consequences ; even to the evacuation of the humours of the eye, and the total loss of the organ. In all operations upon the eye, moreover, the surgeon has to contend against a moveable eye-ball, and the voluntary and involuntary contractions of the orbicularis muscle, and the space he is to operate in is very limited, and more or less deepened by the prominences of bone which form the natural defences to the contents of the orbit.

"The patient being thus prepared, and the eye-lids held apart, the operator commences to make the required section of the conjunctiva. In some of my early operations I accomplished this purpose with a forceps and a narrow-bladed, sharp-pointed knife ; but further experience has shown me that it can be better and more quickly performed by means of the sharp-pointed hook, and a common sharp-pointed pair of scissors than by any other instrument.

"The forceps is apt to let go its hold of the conjunctiva, and pinching this membrane always induces a considerable degree of pain, which the punctures made by the hook never do.

" By having the hook fixed in a cedar handle, a great degree of lightness is given to the instrument, so that if the patient should start suddenly, or in any other way struggle so as to interfere with the operation, the operator may let go the instrument while it is yet fixed in the conjunctiva, without doing injury to this membrane, endangering the eye itself, or losing its hold. In three or four operations in which the patients struggled, I found the advantage of this precaution.

" The operator takes the hook in his right hand, and inserts it into the tunica conjunctiva, about two lines or two lines and a-half distant from the cornea, and on a line corresponding to its transverse axis. He then transfers the hook to his left hand, and taking in his right the sharp-pointed scissors, he first gently draws forward the conjunctiva, and makes a semicircular incision of this membrane from below upwards, upon the outer side of the hook, to an extent varying from four to six lines. It may sometimes even be desirable to make the incision longer. If the patient feels faint at this time, or if he gets pale, the hæmorrhage from the incision of the conjunctiva will hardly amount to more than a drop of blood; indeed, in two cases where the patient felt faint, there was not during the entire operation, as much blood as would soil a white handkerchief; but two hours after the operation, when the system reacted, three or four drops of blood escaped from the incision.

" In the majority of cases, however, the hæmorrhage will amount to some eight or ten drops, which will interfere more or less with the future steps of the operation. When this occurs the operation had better be suspended for a moment or two, and the blood be removed by means of a fine sponge and tepid water, when the bleeding will soon cease.

" The advantages which obtain from making the section of the conjunctiva nearer to the cornea, in preference to the plica semilunaris and lachrymal caruncle, are that the membrane is less vascular in the former situation, and the chances of hæmorrhage are diminished in consequence—that the lachrymal apparatus at the inner canthus of the eye is thus protected from immediate injury, or the remote ill consequences of inflammation, should it supervene, that the tendon of the inner rectus muscle is as readily got at in the one situation as in the other, and that the mark of the incision, some weeks after the operation, is scarcely perceptible.

" When the required section of the conjunctiva is made, the fine sharp-pointed hook should be withdrawn, the eye-lids be permitted to meet, and the further progress of the operation be suspended for a few moments; during which time, if there be any hæmorrhage, the sponge and tepid water are to be used; indeed, whether there be hæmorrhage or not, the application of tepid water is always agreeable to the patient. The eye-lids are next to be carefully wiped dry with a handkerchief, and the assistant is to secure them as before, when the lips of the incision of the conjunctiva will be found to be more or less separated from each other. The operator then takes the blunt hook in his right hand, and, inserting it between the edges of the divided conjunctiva, he insinuates it from below upwards, between the tendon of the muscle and the sclerotica. This being effected, either totally or partially, the eye-ball and the muscle are at the complete command of the operator.

" It sometimes happens that upon exposing the incision of the conjunctiva its edges will be found raised in consequence of the blood insinuating itself in the cellular tissue ; if this should offer any difficulty to the introduction of the blunt hook, the lids had better be again closed, and slight pressure be made upon them with the finger over the incision, which will have the effect of displacing the effused fluid. It is very seldom, however, that this effusion takes place, unless the conjunctiva be needlessly pulled or torn.

" The cellular tissue immediately behind the conjunctiva and the sub-conjunctival and sub-muscular fasciæ,. often afford resistance to the passage of the blunt hook. The former can be easily separated with the point of the instrument, and the fasciæ also, but with more difficulty.

" The blunt hook being inserted beneath the muscle, the operator transfers it to his left hand, and having brought the tendon into view, he divides it with a sharp pointed pair of scissors held in his right hand, as close to its insertion as is compatible with the safety of the sclerotica.

" The further proceedings of the operation are simple. If the eye be not immediately everted, or if the patient has the power of turning it so far inwards as to conceal any portion of the cornea, or to hide completely the white of the eye between the cornea and the inner canthus, in all probability some of the tendinous fibres of the muscle have not been divided. To ascertain if this be the case, I have found the blunt hook the best instrument ; it should again be inserted between the edges of the incision of the conjunctiva, and by directing its point upwards and downwards, keeping it at the same time close to the sclerotica, the most minute tendinous filament, if undivided, can be detected, and its section accomplished." —Pp. 67–73.

The after-treatment is very simple ; and in general, in the course of from fourteen days to one month, the patient can use the eye without squinting, and vision is pretty good. In a certain number of cases, however, though the operation is well performed, some degree of squinting remains. This is chiefly observed in long-continued cases.

" When in internal squinting," says Mr Guthrie, " the operation does not perfectly succeed, the eye is sometimes perceived to resume a little of its original cast, or squint, from the eighth to the fourteenth day, or about the time, I conceive, at which the muscle becomes re-united to the ball of the eye, or to some part within the orbit, and considerable anxiety is felt by both surgeon and patient at its occurrence. It does not however follow, that the operation will not eventually succeed, and the eye be restored to its proper place by the efforts of nature alone. I have seen it do so several times, nevertheless, I do not now trust to the unassisted efforts of nature when I perceive the slightest turn beginning, but direct the other eye to be padded, or kept closed, so that its vision may be from time to time effectually prevented, and the patient to use, or to try to use, the remaining muscles. I do this, I believe, with success, even when the eye has a turn in the opposite direction after the operation, relying on the efforts nature will make to bring the axis of the single eye actually in use, (whether the other is covered or not,) into its proper place, even although one of the muscles, usually supposed to effect this object, should have been divided; and this education of the eye is of great service, even where the padding or closing of the other is omitted.

"In some obstinate cases of long standing, the eye, almost immediately after the division of the muscle, returns to nearly its pristine state of obliquity, and although the division and separation of the muscle and its attachment in every direction, in the manner I have especially indicated, until the outer or sclerotic coat is fully exposed to a considerable extent, enables us to succeed, in most instances, in effecting the object in view, it does not always do so; and in a few cases I have been obliged to desist without bringing the eye to an exact central position; or I have found it a little turned inwards on the next or some other succeeding morning."—P. 4.

This is undoubtedly a very neat and useful operation; and though it may not in all cases realize the expectations either of the patient or the surgeon, it is one of the most precise and most bloodless presents which anatomy has for a long time conferred. When we consider the frequency of squinting, the deformity which it causes, and the disagreeable impression it makes on the beholder, it may be well said, that the operation which removes the defect, though not equal in value to one which saves or prolongs life, is, nevertheless, entitled to the next highest consideration, as one which adds much to its comfort.

ART. VIII.—1. *Manual of General Anatomy.* By J. F. MECKEL, Professor of Anatomy at Halle, &c. Translated from the German into French, with Additions and Notes. By A. J. L. JOURDAN and G. BRESCHET. Translated from the French, with Notes, by A. S. DOANE, A. M., M. D., and others. London, 1837. 16mo. Pp. 421.

2. *The Anatomist's Manual; or a Treatise on the manner of preparing all the Parts of Anatomy; followed by a complete Description of these Parts.* By J. P. MAYGRIER, M. D. P., Professor of Anatomy and Physiology, &c. &c. Translated from the last French edition. London, 1839. 16mo. Pp. 564.

3. *The Anatomist's Vade Mecum: A System of Human Anatomy.* By ERASMUS WILSON. With upwards of 150 Illustrations by BAGG. London, 1840. 16mo. Pp. 551.

4. *Demonstrations of Anatomy; being a Guide to the Dissection of the Human Body.* By GEORGE VINER ELLIS, one of the Demonstrators of Anatomy in University College. London, 1840. 12mo. Pp. 620.

THE System of General Anatomy by Professor Meckel, like all the works of that writer, is, for its extent, the most precise, complete, and instructive yet published on that difficult and useful department of anatomy. More systematic in arrangement than that of Bichat, more methodic and logical in its general character, and more recent in its descriptive details,—it is a work which every physiological and pathological reader must study with care if he wishes to form correct ideas on the distribution and properties of the organized tissues of which the human body consists.

It is singular that a work so useful should not sooner have been translated into several of the European languages. The French translation answered well for many readers; but a good English translation could not fail to be an acquisition.

By a good English translation, however, we understand not a translation of the French, but of the German edition; as it is unreasonable to expect that a translation of a translation can be so good as a translation of an original. We regret, therefore, that the authors of the present performance did not at once begin their undertaking with the original, as they must have thus produced a work which would have appeared with higher authority than the present can possibly possess. In the meantime, this may give the merely English reader a general idea of the nature of Professor Meckel's Treatise, and may be useful in directing his attention to the literary history of general anatomy, and to the works of the authors by whom that science has been principally cultivated.

The other works in this list are devoted chiefly to descriptive anatomy, and especially that form of descriptive anatomy which forms the subject of study in the dissecting-room.

The first, viz. the Manual of Maygrier, has been for some time known as a good guide in the dissection and exposition of the human body; and as such it has for some time past been much used in Paris.

Its arrangement, however, is different from that of most manuals; and, instead of choosing the order observed in dissecting the body, M. Maygrier has followed that of the systematic treatises, and thus gives his work a more regular and methodical form.

After some preliminary observations, he treats of the best mode of preparing the bones, and then gives a full view of skeletology, including under this head not only the description of the bones, but of the articulations, the cartilages, and fibro-cartilages, the periosteum, marrow, and synovia.

Myology follows next; and after some directions on the art of dissecting and exposing the muscles, he describes them according to their different regions.

Under the next head, viz. that of splanchnology, the author describes the mode of exposing, demonstrating, and showing the different internal organs, and the cavities formed by them.

Then follow in succession angiology, treating of the arteries and veins, neurology, and the account of the manner of exposing, injecting, and studying the course and distribution of the lymphatics.

This manual contains nothing new. But it gives clear, correct, and intelligible views of the different parts as they require to be studied; and it is a safe and useful guide in the dissecting-room.

Somewhat different, both in object, and also in the mode of its arrangement, is the work of Mr Wilson. Intended as a system of Descriptive Anatomy, he treats of the structure of the human body in successive chapters, under the divisions of Osteology, Syndesmology, the Muscles, the Fasciae, the Arteries, the Veins, the Lymphatics, the Nervous System, the Organs of Sense, the Viscera or Internal Organs, and the Fœtus.

The author has been at great pains to give correct descriptions of all these divisions, and of all organs and textures included under them, in clear and perspicuous language, and he has carefully introduced, so far as was practicable, all the recent rectifications in the anatomy of the nervous system, the vascular system, and other important organs.

These, it will be admitted, are not trifling recommendations of this useful little work. It possesses another which is in a great measure peculiar. The illustrations, with which the descriptions are accompanied, are the most beautiful and finished specimens of correct engraving on wood we have anywhere seen. The objects represented are well chosen and well drawn, and engraved with extreme neatness and delicacy, and so as to give most faithful representations of the objects intended. Indeed, between descriptions clear, correct, and condensed, yet sufficiently minute and detailed for the student, and illustrative figures remarkable for beauty and accuracy, this little work may be recommended as one of great utility to the student, and particularly deserving his attention. Too often mere manuals are of an inferior description as to their contents and arrangement. We are happy to perceive that this makes an honourable exception to the rule; and to assure our readers that they must not confound this production with the other performances of the same denomination.

Of the last publication in the foregoing list, only a part, though evidently the larger part, is as yet published; and, according to the rule by which we are generally guided in noticing works of this nature, it is premature to speak of it at the present time. We have nevertheless been so strongly impressed in looking over its pages with an opinion of its merits, that we think it right on the present occasion to advert to them shortly.

It is not exactly like the work of Mr Wilson, a compendious system of anatomical knowledge. It rather resembles that of M. Maygrier, in being a companion to the labours of the dissecting-room. From these, however, it differs so much in the methodical manner of its arrangement, and in the fulness, amplitude, and minuteness of its descriptions, that we must express our opinion, that while it is superior to any of the dissecting-room manuals or guides known to us, and equal to most of the systematic treatises in ordinary use; and it may be safely recommended to the student, as capable, with dissection and demonstration, of communicating full and correct ideas on the structure, position, and relations of the organs of the human frame.

The arrangement adopted by Mr Ellis is decidedly that suited for demonstration. He begins with the account of the method of exposing, demonstrating, and studying the parts forming the head and neck; and, commencing with the superficial or most accessible parts, he advances successively to the deep-seated and more inaccessible organs.

The arrangement, it may be perceived, is perhaps not so methodical as if the contents of the work had been thrown into the systematic form; and this is probably the only blemish in a work otherwise remarkable for the perspicuity, neatness, minuteness, and accuracy of the descriptions. It must, however, be added, that this fault is inseparable from the mode of arrangement adopted; and it is more than compensated by the advantages conferred upon the student in guiding him in his dissections and in his study of the different parts.

This book it is almost superfluous to recommend. Its merit will do that with little adventitious aid.

ART. IX.—*On the Influence of Artificial Light in causing impaired vision, and on some methods of preventing or lessening its injurious action on the Eye.* By JAMES HUNTER, M.D., Surgeon to the Eye Dispensary of Edinburgh. 8vo. Pp. 94. Edinburgh, 1840.

THE author of this short tract explains, with much ingenuity as well as scientific knowledge, a cause of impaired vision very extensive in operation, and which, from the very general use of gas-light, is likely to become still more so.

Common day-light, which is the only natural light, and the least injurious to the eyes, consists of red, yellow, and blue rays in the ratio of 5,3, and 8; that is a predominance of blue rays, which form the mildest and least hurtful to the retina. In the ordinary forms of artificial light, as the light from candles, either of tallow or wax, oil or coal gas, this proportion is reversed, the yellow and the red rays being in excess, and the blue rays in the ratio of minority. From this peculiarity in artificial light in the excess of red and yellow rays, various inconveniences and evils result.

1. Colours and coloured objects appear in it differently from what they do by day-light. 2. The retina is unequally, we would say too strongly excited, and becomes less sensible to those rays that are in excess, so that afterwards, when it views a white object by day-light, the blue rays contained in the white light reflected from its surface, make a greater impression than the red or yellow rays, and the objects assume more or less of a dingy blue or purple colour. This over-excitement of the retina produces films, *muscæ volitantes*, and even amaurosis. 3. The red and yellow rays possess considerably higher heating powers than the blue rays, and therefore artificial light, which contains these calorific rays in larger proportion, must be injurious to the retina in this manner. 4. From artificial light furnishing less defining power, it is requisite to use it in considerable quantity; and this is another source of over-excitement to the retina. 5. While, in the case of natural or day-light, the ordinary calorific or heating powers of the red and yellow rays are, by various circumstances and beautiful provisions in its transmission through the atmosphere, almost annihilated, so that these rays arrive at the eye with much less of their heating powers than they would have, if direct, the rays of artificial light, by coming more directly and in a more concentrated form on the eye, are accompanied with their concomitant heat, and thereby injure the retina greatly by this hurtful adjunct. 6. The carbonic acid gas which is extricated from all artificial lights, procured as they are by combustion, though not acting on the eye itself, acts on the brain, and especially the optic nerves, the sensibility of which it weakens and ultimately destroys. 7. All artificial lights are unsteady and irregular, and very often placed in improper positions.

Of all artificial lights, gas light is the most hurtful, not from anything peculiarly noxious in its qualities, but from its cheapness, which leads to its being used in excess, and, consequently, in such quantity as to over-stimulate the eyes.

To obviate these several evils, Dr Hunter recommends the more sparing use of gas light, and other artificial lights, and the employment of means calculated either to add blue rays when deficient, or to withdraw the red and yellow rays when in excess.

This object is to be attained by the use of conical reflectors of a bluish or azure colour, and chimneys stained of a blue colour. Pale blue glasses are also recommended; but Dr Hunter thinks their use is not advisable, as they become hot, and require, from the loss of light they cause, to be often removed.

This tract is entitled to general attention.

PART III.

MEDICAL INTELLIGENCE.

PHYSIOLOGY.

On the Functions of the Ganglionic Nerves. By Dr **Remak** of Berlin. (*Ammon's Monatschrift*, June 1840.)—Every nerve is composed of an assemblage of primary filaments, which do not communicate together, even in the most compact plexus, but are only placed in juxtaposition, taking their origin either from the cerebro-spinal axis or ganglia, and ending in the organs to which they are distributed.

The nerves which communicate with the cerebro-spinal axis have two orders of roots, the anterior, being those of motion, the posterior, those of sensation, the only exceptions to this being the optic, the olfactory, and the auditory nerves. M. Ehrenberg was the first to demonstrate by microscopical researches, that in every nerve the motor filaments could be easily distinguished from those of sensation; the latter assuming a varicose, moniliform, or necklace aspect, whilst the former remained of a cylindrical form, and were only somewhat rugous. M. Ehrenberg thought that the primary filaments of the nerves were hollow, and contained a species of soft medullary matter, which could be compressed by the contraction of the walls of the tube. M. Remak, however, does not think that these tubes contain a medullary matter; but he says he has observed in their interior a kind of strengthening band, running in a longitudinal direction, and similar to the longitudinal bands of muscular fibres of the large intestines. When this band contracts, it gives to the primitive nervous tube or fibre a more or less puckered appearance. The walls of the tube of the sensitive nervous filaments are excessively thin. This is the reason why it assumes a varicose aspect when its band contracts. In the motor filaments, the walls are more resistant; the tubes, therefore, preserve a cylindrical form, and the contraction of the band only causes it to assume a rugous aspect. The thickness of the walls of the motor filaments is also the reason of their greater opacity, greater dimensions, and being more visible than the sensitive filaments; their lateral-rounded margins are also distinctly perceived under the microscope, whilst those of the sensitive filaments appear flat; these last, indeed, are so transparent, that it requires a very feeble light to see them distinctly.

Such, then, are the characters by which M. Remak is enabled to distinguish the sensitive from the motor filaments. He has also discovered that these nervous filaments may be distinguished from those of organic life by being always of a white colour, whilst those of organic life are of a red colour, and extremely fine, even more slender than the sensitive filaments of the spinal cord.

The nerves of organic life owe their red colour to a medullary matter, which exists in the ganglia from which they appear to originate; the ganglia of the great sympathetic are crossed by the nerves of the cerebro-spinal axis, which passes through them without confounding themselves with them.

In every nervous bundle the three kinds of primary nervous filaments, viz. motor, sensitive, and organic, are usually met with. The organic filaments, which proceed from the ganglia backwards to the spinal marrow, become more and more slender as they recede from the ganglia, till they are finally lost in the substance of the cerebro-spinal axis, as in other organs. The organic filaments which unite with the fifth and sixth pairs of cerebral nerves also decrease in size, in proportion to their distance from the superior cervical ganglia, from which they originate.

The filaments which unite the ganglia of the grand sympathetic in front of the spine and ribs are white, and contain but few primitive tubes or nerves of organic life. The posterior branches of communication are also white, but contain many more primitive red tubes of organic life.

The nerves which proceed from the ganglia forwards to the organs of the body present the greatest mixture of red and white filaments; and the number of red filaments is proportionally greater, as the organs are less capable of being acted on by the will, as is seen in those distributed to the liver and spleen. Those nerves of the great sympathetic which contain many white filaments, as the great splanchnic, transmit also to the *sensorium commune* the sensations of impressions made on the organs in which they terminate. These same nerves communicate also volition; for certain individuals, and M. Remak is of this number, possess the power of increasing the peristaltic movement of the intestines, without calling in the aid of the abdominal parietes.

In conclusion, M. Remak remarks, that, in the cerebro-spinal system of nerves, two orders of action take place,—the perception of sensations, and the reactions of volition. Two analogous actions take place in organic life, organic perception, or, as it has been called, Hallerian irritability, and reaction, or the function of organic reflection, so well demonstrated by Muller. From this, he thinks that the animal economy is justly described as possessing two sensoriums, the one, that of the cerebro-spinal axis, the other, that of the ganglionic system.

Acidity of the Menstrual Secretion the cause of its non-coagulability. By M. Retzius. (*Journal für Geburtshülfe, de Siebold,* Vol. 17.)—The absence of fibrine in the menstrual blood has been generally regarded as the cause of its not possessing the power of coagulation. M. Retzius, as well as M. John Müller, have carefully examined this secretion, and has asserted that its non-coagulability depends on the presence of free phosphoric and lactic acids. They besides observed that the blood globules were of the same size and form as in healthy blood, and equally numerous

M. Retzius believed that these acids were developed in the spermatic and uterine arteries, in which a kind of congestion occurred previous to menstruation; that the acids then mixed with the blood and prevented the fibrine from coagulating. If consequently, says M. Retzius, the menstrual secretion be abundant, the last portions of the secretion ought to be alkaline and coagulable, because the whole quantity of the acid is by that time thrown off; a circumstance which, he says, he has observed in a case of metrorrhagy the fourth day of menstruation.

MORBID ANATOMY AND PATHOLOGY.

Case of Irregular Disposition of the Aorta and Pulmonary Artery. By M. F. Ducrest. (*Archives Generales de Medecine,* Sept. 1840.)—A female child was born at the Hospital of Maternité on the 8th of June 1840, in whom respiration was never perfectly established; the infant never uttered a single cry, and refused the breast as well as all liquids which were offered to it. The skin of the child was of a pretty deep blue colour, and, notwithstanding extreme weakness and these unfavourable appearances, it lived for ten hours after its birth.

The body of the child was fully developed; the limbs were well proportioned; and it presented in every respect the appearance of a child born at the full period.

Each lung presented a double fissure, dividing them into three lobes. When removed from the body alongst with the heart, and thrown into water, they floated and appeared to be healthy. The great blood-vessels, however, presented the following singular distribution. The aorta took its origin from the right ventricle, and gave off in the usual manner the two cardiac coronary arteries. The trunk of the pulmonary artery rose from the left ventricle, and, after a short course to the left of the aorta, divided into three branches, two going to the lungs, the third representing the *canalis arteriosus* uniting with the aorta towards the termination of its curvature. All these vessels were of their usual proportions. The ventricle from which the aorta originated, besides its position on the right side, exhibited all the usual characters which distinguish it from the left ventricle. The two *venæ cavæ* and the large cardiac vein terminated in the right auricle. The left auricle received the four pulmonary veins. The *foramen ovale* was furnished with a perfect valve, which opened from the right to the left auricle. The septum between the ventricles was complete. No other unusual appearance was met with in any other part of the body.

Case of Absence of the Pulmonary Artery. By M. Bigger. (*Wochenschrift fur die Gesammte Heilkunde,* 1839, No. 36.)—An infant which, from the fifth day after birth, had been affected with cyanosis, died at the age of five months and a-half, of convulsions and dyspnœa. The left auricle and ventricle of the heart were found to be much smaller than those of the right side. The *foramen ovale* was still patent, but was closed with a perfect valve. The pulmonary veins were much contracted, but no traces of the pulmonary arteries could be detected. The aorta arose from the septum of the two ventricles, and communicated with both, so that it received the blood from both sides of the heart. A considerable arterial branch, about the size of the *arteria innominata,* was given off from the arch of the aorta, and was distributed to the lungs.

Case of Congenital Hernia of the Heart and Stomach. By M. Becker. (*Medicinische Zeitung,* 1839. No. 16.)—A child was born in March 1839, well formed, vigorous, apparently quite healthy, and was still alive when the account was published. It was affected with a hernia of the heart and stomach, which protruded from the median line beyond the thorax and abdomen. The sac in which they were enclosed was almost transparent, and allowed the included organs to be pretty distinctly seen, as well as a septum which separated the heart from the stomach. It was about three and a-half inches long, by four inches broad, and was neither covered with muscles, bones, nor true cuticle, but appeared to consist of a peculiar membranous expansion. The bony and muscular parts, which usually cover and protect the heart and stomach, were awanting in this case over the median line, and had thus allowed of the protrusion of these organs.

On the Developement of Cancer in the Veins, and the Transmission of Cancer from Man to the Lower Animals. By Dr Langenbeck. (*Gazette Medicale de Paris,* 19th September 1840.)—The cases in which cancerous matter is met with in the veins are of such frequent occurrence that Cruveilhier was induced to conclude that all cancers were originally developed in the capillary system, and the recent researches of Dr Quain and Dr Hake on the varicose capillaries, tend to establish the same fact. Dr Langenbeck, however, is inclined to doubt the correctness of this statement, as he has not been able to trace the origin of the disease to the vascular system at that period when it was but commencing, and was invisible or nearly so to the naked eye. He, therefore, regards the cancerous masses which are met with in the veins at a more advanced period of the disease, as the consequence of the diseased action, and analogous to the phlebitis which so frequently complicates or attends cancer of the uterus.

But although he considers that cancer rarely originates in the capillary system, yet he thinks it is by means of the capillaries that it is propagated to other parts of the body, as to the lungs, after it has originated in the uterus.

The cancerous matter which Dr Langenbeck found in the veins was in three different stages: *1st*, Quite unconnected with the coats of the vessels; *2d*, Slightly adherent; and *3d*, Strongly adherent, forming an uniform mass with the coats of the veins, which appeared to be transformed into cancerous matter. From comparing these appearances with the progress of the disease, as it occurs in the lungs consecutive to cancer of the uterus, he has arrived at the conclusion, that the microscopic cancerous molecules or cellules, when completely isolated and introduced into the blood, possess the power of propagating the cancerous disease; in fact, that the developement of cancer depended, like that of all organized products, on the multiplication of cellules; and that a cancerous cellule once introduced into the circulating system, could become developed and produce cancer, in the same way as an isolated utricle detached from an individual of one of the lower orders of plants could reproduce that plant.

Dr Langenbeck thence infers, that cancerous cellules can enter the circulating fluid in three different ways:

1. The cancerous matter developes itself in the blood, passes to the state of cells, and fixes itself in an organ where it forms a tumour.

2. When cancer is once developed in an organ, the veins and lymphatic vessels take up the characteristic liquid of cancer and pour it into the circulation where the cellules multiply, and afterwards are thrown into some organ or other of the body.

3. In an ulcerated cancer of the uterus, for example, the lymphatics and veins being destroyed, the cancerous cellules are quickly taken up, but, being arrested in some point of their course, such as in the thoracic duct, or the capillaries of the lungs, quickly form cancerous masses.

The first of these is not admitted by all pathologists; but Dr Langenbeck asserts that he was enabled to ascertain its correctness from the microscopic examination of the blood of cancerous subjects. He gives examples, however, of the third form. In two cases of cancer of the uterus, he was enabled to exhibit the cancerous cellules. The veins of the uterus and of the pelvis were filled with coagulated masses of a reddish-yellow colour, composed of fibrine, globules of purulent matter, and little carcinomatous cellules, the diameter of which was double that of pus. These carcinomatous masses were formed chiefly of elliptical transparent globules, similar in every respect to the granular substances which constitute the original carcinomatous cellules, and of which they are the contents. The iliac veins, the *vena cava* inferior, and the right side of the heart, were filled with black and liquid blood, in which was also found carcinomatous cellules, and granules similar to those described above: and blood from the left side of the heart contained clots enclosing the same kind of matters. In the pulmonary artery these carcinomatous matters were adherent here and there to its coats, and filled completely some of its smaller ramifications. These matters were composed almost solely of cancerous cellules, the most of which had a diameter five or six times greater than the globules of the blood, and were strongly adherent to the walls of the vessels. They were very different, therefore, from the cancerous matter found in the iliac veins, which were composed almost entirely of globules or granules, and were quite free. When the ramifications of the pulmonary artery, which were filled with the cancerous matters, were traced, they were found to terminate in sub-pleural tumours of the same nature in which all the tissues were confounded in one morbid mass.

Dr Langenbeck thinks that all secondary carcinomatous tumours of the lungs are formed in this manner.

He endeavoured by experiment to ascertain whether the primitive carcinomatous cellules introduced into the circulation of another individual could produce cancer. He therefore injected into the veins of rabbits cancerous matter extracted from two breasts newly amputated. The whole of them died at the end of from twelve to twenty-four hours with symptoms of pulmonic oppression; and their lungs were found filled with ecchymosed spots.

But the most interesting and satisfactory experiment on this subject was the following: Sixty-two drachms of blood were drawn from the femoral artery of a dog deprived of its fibrine, and mixed with eight drachms of cancerous matter, obtained from an enormous cancer of the uterus, and was again injected into the femoral vein of the same dog. Dyspnœa was at first produced, but gradually subsided. Feverish symptoms then supervened; but by the end of eight days the dog appeared to be quite recovered. It got thinner, however, though its appetite continued good. It was killed at the end of two months, and there was discovered at the anterior part of the upper lobes of the lungs of both sides two or three flat grayish-coloured tumours of the size of a lentil, similar in all their characters to the carcinomatous tumours of the lungs in man. In the middle lobe of the right lung a hard circumscribed tumour of the size of a haricot bean, and having all the characters of a carcinomatous tumour, was met with. When a section of it was made, it was found to be composed of a hard, homogeneous, bluish-coloured substance, interspersed with red points. When subjected to the microscope, it presented the appearance of strong fibres of the diameter of the primary muscular fibres, intermixed with cellules about the one-hundreth of a line in diameter. The liquid expressed from the tumour contained globules of the size of those of the blood, as well as others much smaller, and also globules of fat, substances which Dr Langenbeck had already discovered in the medullary sarcomatous tumours of man.

Sudden Death from Rupture of the Spermatic Vein. By Dr M'Naughton. (*American Journal of the Medical Sciences*, August 1840.)—The subject was a female, 38 years of age, who had always enjoyed good health. During the night of the 27th January, she did not sleep well, was troubled with dreams and uneasy feelings in the bowels. The next morning she made a hearty breakfast; but, the uneasy sensations increasing, she swallowed a dose of salts, which were instantly vomited. The pain rapidly increased in severity, and a physician was sent for. He found her very pale with a cold surface; the lips and tongue pale; and complaining of pain through the abdomen and thorax; with difficulty of breathing and faintness and a sensation of bearing down. Her pulse was small and weak. Calomel and opium were ordered in divided doses.

As her symptoms increased in severity, another physician was called in, when a round elastic tumour, nearly two inches in diameter, was discovered near the umbilicus. This tumour the patient said had existed for fifteen or sixteen years, and could not be the cause of the present symptoms. A catheter introduced into the bladder drew off very little urine.

Dr M'Naughton was then added to the consultation. He found the patient cold, pale, and almost pulseless, complaining of pain in the lower part of the pelvis, and having urgent desire to void urine. The abdomen was slightly tumid but not tense, with general uneasiness and great tenderness in the region of the navel. Close to this part there was a hernial tumour, which gave great pain even on the slightest pressure. Believing that the tumour was the cause of most of the bad symptoms, Dr M'Naughton thought it prudent to reduce it, and succeeded in doing so by gentle steady pressure, in a few minutes, the patient all the time complaining most bitterly of the pain. The reduction of the tumour was followed by sickness and vomiting; after which she became somewhat easier, and her pulse rose a little.

The next morning her hands were somewhat warmer, and pulse more distinct; her face was pale, cold, and œdematous. At the umbilical foramen there was a soft tumour, less than an inch in diameter, resembling an omental hernia; and, as her medical attendants had still some suspicion that her symptoms were connected with this tumour, they determined to cut down upon it. The tumour was not strangulated, and consisted of a cluster of masses of fat of different sizes. The operation did not afford any material relief, and the state of the protrusion was not such as would satisfactorily account for the intensity of the prostration. The sensation of bearing down, and the urgent desire to void urine continued. The uterus was low in the pelvis, and morbidly sensible to the touch, but not larger than is usual in the unimpregnated uterus of a person who has borne children.

In the evening the symptoms appeared milder, and an enema, followed by a full dose of calomel, was administered.

On the morning of the 30th, she seemed somewhat better; but in the course of the day, her distress returned, and she gradually sunk till the morning of the 31st, when she expired.

The tumour which appeared at the umbilicus, and which was reduced by the operation, proved to have no connection with the omentum, but to be merely a mass of adipose matter external to the peritoneum, filling up the umbilical foramen. Half an inch below there was a small opening in the *linea alba*, through which the herniary tumour, which had been reduced by the taxis, had protruded. Well-formed purulent matter was found at the umbilicus, beneath the abdominal tendon, but external to the peritoneum; the peritoneum, however, appeared healthy, and free from inflammation. The omentum and intestines in the vicinity of the umbilicus were healthy in appearance, and in their proper relative positions, but both the small and large intestines were enormously distended with flatus. The interstices between the intestines were occupied by bloody serum, and the whole pelvis was filled with a coagulum of venous blood, which, on removal, filled a large chamber-pot. The source of this hæmorrhage was traced to the right spermatic vein but Dr M'Naughton has neglected to describe the condition of this vessel. The ovaria were shrivelled and hard. The upper part of the jejunum appeared dark for more than eighteen inches, and had evidently lost its vitality, as it readily gave way, and was much more easily torn than the intestine, either above or below the darker portion. It was concluded that this was the portion which had been protruded through the opening in the *linea alba*, and was the cause of the tenderness about the navel, the vomiting, &c. There was no appearance of pregnancy, either uterine or extra-uterine. That the intestines were not thickened nor the peritoneum more inflamed, might be accounted for by the exhausted state of the vascular system not admitting of the occurrence of acute inflammatory action.

On the occurrence of Variolous Pustules on the Internal parts of the Body. By Professor **Alexander** of Utrecht. (*Gazette Medicale de Paris*, 26th September 1840.)—Since 1830, variola, varicella, and the varioloid disease, have been very prevalent at Utrecht, and Professor Alexander availed himself of the opportunity of examining into the frequency of the occurrence of variolous pustules in the interior of the body. From his researches, it appears that variolous pustules occur on the tongue, and on the velum and arch of the palate, but not on the trachea or œsophagus. He, however, has in his possession a preparation of the trachea on which the marks of a variolous pustule may be detected. In every case, however, traces of inflammatory action were observed both in the trachea and œsophagus, and in many coatings of plastic lymph covered the surfaces of the lining membrane. In the intestines of those who had died of small-pox, he has often seen pustules and ulcerations, but precisely similar to those which are met with in those who have died of typhoid fever and pulmonary consumption. These pustules, he says, cannot be compared to those of variola, since there exists no epithelium in the intestines; they appear rather to be enlarged or engorged intestinal glands, the orifice of which is swelled, and appears under the form of a dark point.

On the Exanthemata and Enanthemata. By Dr **Eisenmann.** (*Hufeland's Journal der Practischen Heilkunde*, March 1840.)—Linné appears to have been the first to point out the analogy which exists between the cutaneous eruptions and those of the mucous membranes. But since the publication of the works of Roederer, Broussais, Bretonneau, Louis, Cruveilhier, Andral, &c. this analogy may be regarded as proved. The name exanthemata has therefore been applied to those eruptions, and they, like the exanthemata, constitute characteristic symptoms of special maladies. Dr Eisenmann, in his long and elaborate essay, proves the perfect similarity of the symptoms, progress, and termination of the eruptions which occur on the mucous membrane with those seen on the cuticular surface. He shows that pustular, vesicular, erythematous, &c. eruptions are common to both surfaces; that the termination in suppuration, resolution, desquamation, &c. equally occur in both forms; the last, indeed, desquamation, being now clearly proved by the researches of Boehm and Henle, who discovered the debris of the epithelium of the intestines amongst the mucous and feculent matters passed during the continuance of these diseases. He also shows that these exanthemata are contagious and epidemic, the same as the exanthemata.

M. Eisenmann divides the enanthemata into five genera, to which he has in the meantime added a sixth, to include an anomalous form of disease. The following are the genera he describes:—

1. Erythema of the mucous membrane, terminating in desquamation, a form of disease distinctly described by Boehm and Henle.

2. Vesicular eruption of the mucous membrane; observed and described by Jahn, as occurring on the palatine arches during measles.

3. Papulæ of the mucous membrane. Several species of this form have been described, as aphthæ, the papular eruptions seen on the throat and fauces by Rœderer in gastric fever, and those seen so commonly on the same parts during the influenza of 1837.

4. Pustules of the mucous membrane, as seen in the aphthæ and eruptions of the mucous membrane in dothin-enteritis.

5. Fungi or tubercles of the mucous membrane, as has been frequently seen and described in typhoid fevers, in malignant puerperal fevers on the vaginal mucous membranes, common in dysentery, and in diphtheral inflammation of Bretonneau.

6. The eruptions which have not yet been classed, as the patches described by Boehm in the neighbourhood of the ileo-cæcal valve, and which seemed to be formed by a morbid state of the glands of Peyer.

On Rabies. By M. Breschet. (*Seances de l'Academie Royale des Sciences*, 21st September 1840.)—Doubts have lately been raised as to the contagious nature of the poison of rabies, and M. Breschet, in order to determine this point, read a paper before the Royal Academy of Sciences, detailing experiments which he had made with M. Dupuytren thirty-five years ago, which seem to prove beyond a doubt the contagious nature of the poison.

Some diseases proper to one class of animals are not developed spontaneously in others. Such appears to be the case with rabies, which seems to be peculiar to the genus *Canis*, but more especially to that well known species, the domestic dog. It is from this species that the disease is usually transmitted to others of the Mammalia, and to man.

Rabies in man is never a spontaneous affection, nor the result of a combination of moral or hygienic causes; in every case it may be traced to the inoculation of rabid matter. It cannot be doubted that the reason why hydrophobia and rabies have been confounded arises from having overlooked the difference of those nervous affections where there is exhibited horror at the sight of fluids, difficulty of swallowing, &c., and the true communicated rabies. In this last affection, art, unfortunately, is never of any avail: whilst the symptomatic hydrophobia of the pernicious hydrophobic fevers, for example, is not necessarily fatal,—or if so, only in consequence of a peculiar concatenation of circumstances.

Such are M. Breschet's views, founded on the following experiments:—MM. Breschet and Magendie collected a quantity of the saliva of a man who died of rabies, and introduced it under the skin on the dorsal region of a dog. Thirty-eight days after this inoculation, the animal was affected with violent rabies. It was made to bite other dogs, and these were all similarly affected. In continuing these experiments it was observed that the disease failed to manifest itself, or was only developed with great difficulty when the contagious principle had passed through three or four animals successively; it generally, indeed, failed to produce the disease in the third transmission. If this fact should be confirmed by subsequent experiments, it will prove the singular fact, that this poison loses its deleterious properties by passing from one individual to another of the same species.

M. Breschet, from his experiments, ascertained that, in general, symptoms of rabies came on from the twentieth to the thirtieth day after the animal was bitten. In several cases, however, he has seen three months elapse before rabies made its appearance. In many of the rabid dogs the horror at the sight of water was wanting, and these animals even drink with avidity the water which was presented to them,—a fact which proves beyond a doubt that rabies and hydrophobia are two very different and quite distinct morbid states.

M. Breschet's experiments also prove the communicability of rabies from carnivorous to herbivorous animals. An ass bitten by a mad dog took on all the symptoms of acute rabies at the end of three weeks. Similar results were afforded by sheep, only that in them the disease was less intense.

The foam collected from the mouth of the ass and of other solipeds was introduced by M. Breschet beneath the skin of several dogs, and after a period of incubation of from twenty-five to forty days, produced true rabies. This experiment was several times repeated, and always with the same result; proving the transmissibility of the poison of rabies from herbivorous to carnivorous animals,—a fact denied by several veterinary practitioners.

The saliva of rabid dogs was also inoculated on rabbits and guinea-pigs, and, in almost every instance, after a short period, these animals fell victims to the poison, without, however, exhibiting any symptom usually considered as characteristic of rabies.

Similar experiments on birds of different species, as fowls, ducks, crows, and birds of prey, gave negative results as to the exhibition of the phenomena of rabies. All, however, died rapidly; whilst others, on whom similar wounds had been inflicted, but were not inoculated, continued to live.

M. Breschet also made several experiments to determine whether the blood was altered in rabies. He often attempted to inject the blood of a rabid dog into the veins of a healthy dog, but as this was found to be both a difficult and hazardous operation, after a few attempts it was abandoned. He afterwards obtained the blood of the rabid animal by bleeding, and, after washing it with distilled tepid water, the fluid was injected into the vein of a healthy animal. In all these experiments, though frequently repeated, M. Breschet found it impossible to communicate rabies; and he therefore is inclined to believe that the poisonous matter of rabies, whatever it be, resides in the saliva. The foamy saliva is in reality an altered humour,—a matter in a truly morbid state, and the vehicle of a deleterious principle of a true rabid virus, but the nature of which is still unknown. M. Breschet then regards rabies as a virulent and contagious disease, and not the effect of a moral affection.

MATERIA MEDICA AND THERAPEUTICS.

On the Employment of Blood-letting and Opium in Acute Phlegmasia. By Dr Thomas Barbour. (*American Journal of the Medical Sciences.* August 1840.) The employment of copious blood-letting, followed by the speedy or immediate exhibition of large doses of opium, has been, for many years past, found to be a very efficacious method of removing the symptoms of inflammation when seated in different internal organs. Thus a blood-letting to the extent of 25 or 30 ounces, or even more, followed by a dose of opium to the amount of two grains, or of solution of muriate of morphia to the amount of 50 minims or one drachm, and repeating the opiate in the course of a few hours, has often been more effectual in removing inflammatory symptoms than blood-letting alone. This is particularly the case in pneumonia, pleurisy, peritonitis, nephritis, and rheumatism. It is well known that patients can bear large blood-letting better when followed by opium than when practised alone, and that the exhibition of the opium has the effect of not only relieving pain and bringing down the pulse, but of determining the blood to the skin, and counteracting the nervousness, irritability, and sinking often produced by profuse blood-letting.

Dr Barbour states that he has been very successful in his treatment of acute inflammatory affections, by employing abstraction of blood to the extent of producing a marked effect on the system, and following the sedative action of the bleeding by large doses of opium, repeated according to circumstances, so as to prevent the re-developement of morbid excitability, or that of reaction. His plan of treatment is as follows. If the patient be robust, and the disease of intense character, he makes him stand up, or if otherwise, he makes him sit up, unsupported; he opens a large orifice, so as to admit of the blood escaping in a full stream; keeping the fingers upon the pulse, and directing close attention to it, so as to judge of the slightest change, and inquiring often if the patient feels any nausea, vertigo, or other unpleasant sensations. As soon as the pulse begins to be feeble, and the countenance to become a little pale, he applies the finger over the orifice of the vein, and makes the patient lie down with his head and shoulders low, and if he grow paler or sicker, or syncope threaten, sprinkles the face with cold water, or applies ammonia to the nostrils. If there be a disposition to reaction, the patient should be again made to sit up, and a small quantity of blood allowed to flow from the vein, till the vital powers be again reduced. This is now the time for the free administration of opium in sedative doses, from two to four grains, according to the individual idiosyncrasy. When administered in this large dose, and the system kept under its influence by giving a grain every hour or two, opium will in general be found capable of preventing the re-developement of morbid excitability or reaction.

Such are the means recommended by Dr Barbour for the cure of acute inflammatory attacks. But though his chief reliance is placed on these, he does not trust to them alone. Thus in pneumonia and pleuritis he recommends, as a useful auxiliary, the fourth of a grain of tartar emetic every hour, or a fourth to half-a-grain of calomel in the same period of time; together with the occasional administration of a gentle laxative. When the inflammatory attack supervenes on fever, he considers general blood-letting as improper; but the same beneficial results follow the employment of topical blood-letting, counter-irritation, and the free use of opium, with perhaps an occasional stimulant.

The principle of this treatment is good; but it is not so new as Dr Barbour imagines. The propriety of the mode in which it is performed is very doubtful.

In the first place, though Dr Marshall Hall and various other physicians recommend the detraction of blood in the erect position, as best calculated for producing speedily on the system the conditions favourable to the removal of inflammation, it will often be found that it gives rise to a great fallacy. The faintness thus produced is often merely mental, and is always too temporary and too trifling to facilitate the removal of the congested blood from the vessels of an important organ or membrane in a state of intense inflammation. Blood-letting, to be beneficial, operates not only as a physiological remedy, but by its hydraulic effects upon the whole vascular system, and thereby upon those of the inflamed organ; and, unless the former are relieved of the load with which they are distended, it is impossible that the blood can be withdrawn from the latter, or that their tension and fulness can be diminished. Then comes the physiological effect, which is partly referable to the contractile power of the capillaries of the organ, if not wholly destroyed, and partly to the imbibing and resorbent power of the veins. These effects are always most completely insured by drawing blood in the horizontal position; and by taking as much as is likely to diminish sensibly the tension and fulness of the vascular system. It is chiefly in the state thus produced, when the quantity of blood is so small as not to stimulate in the wonted manner either the heart and arteries or the system at large, that the exhibition of opium in large doses becomes beneficial and curative. It then allays nervous irritability, favours the return of the circulating current to the heart and large vessels, and, by acting on the capillaries, determines the skin, and abates internal determination.

The practice recommended by Dr Barbour, of placing the finger over the orifice in the vein, cannot be too decidedly reprobated and condemned. It is very filthy, very dangerous, and totally unnecessary.

1. It is filthy, because it applies to the lips of the wounded vein the secretions of another person; for there is no finger, be it ever so well washed, that is not covered with perspirable matter, which, when applied to the edges of the wound in the vein, acts as a poison.

2. The practice is very dangerous. The great majority of instances of *inflamed vein* are produced by this foul and pernicious practice of pla-

cing the finger over the orifice of the vein. Authors have perplexed themselves to explain the origin of this dreadful and unmanageable accident, and have ascribed it alternately, to a foul or rusty lancet, to a blunt lancet, to washing the wound with sponges, to dressing it with unclean linen or charpee, and to several other pieces of practice unnatural, unnecessary, or ofttimes dirty. But they seem not to be aware, that the accident most frequently follows this mode either of temporarily suspending the blood-letting, or of closing the vein. We have seen a considerable number of accidents of this kind; and it has always been observed or turned out on inquiry, that the finger was applied once or twice over the wound in the vein. We have, both personally in performing blood-letting observed the rule of never applying the finger over the wound, and in instructing others in its performance we have enjoined the observance of the same rule; and in no instance has it been known that *phlebitis* has followed blood-letting when performed and completed with attention to the rule now specified. Wherever, on the other hand, the practice of placing the finger over the wound becomes general, instances of phlebitis are also common.

We know that it is the custom of many surgical teachers to recommend the practice, and hence it is so frequent. But with this we have nothing to do. The numbers that recommend a piece of bad practice will never make it good, especially in opposition to manifest physiological and pathological facts. If it be argued that, amidst the general use of this method, the cases of phlebitis are not equally general, we reply, that in a certain number of cases, though the finger is placed over the wound, it does not touch the lips of the vein, but only the wound in the skin, and the adipose tissue. But wherever the finger is applied to the lips of the wound in the vein, inflammation will assuredly follow.

The same is true of venous inflammation after amputation and other wounds. What is the cause of the mortality after amputation and other operations in the Hospitals of Paris? Chiefly, we believe, the groping, poking, and rubbing with fingers and charpee over the stump in repressing venous hemorrhage. The surgeons seldom dress the stump, and much more rarely tie the arteries themselves; and as the whole business of securing blood-vessels and dressing after the limb is lopped off is left to pupils, the rough usage is often very great.

3. This rule of placing the finger over the orifice in the vein is totally unnecessary. Any one who understands the circulation, knows that if he wishes to suspend the bleeding from a vein, it may be most effectually done by placing the finger over the vein below the wound; and if he wishes to stop it altogether, he does so most effectually by untying the bandage, and placing the arm and fingers at complete rest, when, after the escape of the little blood left between the wound and the nearest valve, the bleeding ceases entirely. If it proceed, the gentlest pressure over the vein below the wound is quite sufficient to arrest it.

It is not unreasonable to observe, that this practice of placing the finger over the wound in a vein was much favoured by the army surgeons who returned from the peninsula in 1814 and 1816. They had learned that, in arresting the flow of blood from wounded vessels, especially arteries, the first thing to be done was to place the finger immediately over the bleeding orifice; and this rule they accordingly inculcated on all their friends, who in their turn taught it to their pupils, as a rule sanctioned by experience. And a good rule it is as to arteries, but as to veins decidedly bad. So important is it to attend to little and apparently trifling peculiarities. We believe that, to the rule thus heedlessly and wantonly taught and inculcated upon pupils, many lives have been sacrificed. We cannot allow the direction to pass through the pages of this Journal without lifting up a strong, unqualified, and decided testimony against it; and saying to all and sundry who would practise venesection without danger to their patients and without remorse to their own minds—Pray avoid it.

The practice pursued by Dr Barbour, however, is neither new nor peculiar. We remember of seeing more than twenty years ago an eminent hospital physician treat internal inflammation by the exhibition of a large dose of opium after copious blood-letting; we have done so repeatedly; and all experienced physicians know the value of opium in large doses in peritoneal and other internal inflammations.

Regarding the combination of calomel or antimony with opium, still less is that entitled to the character of novelty. Dr Hamilton of Lynn Regis showed the efficacy of calomel and opium, and calomel, antimony, and opium so far back as 1784; and to all those who have read the writings of Dr Armstrong, the practice of giving opium, either alone or with calomel, must be familiar.

On the Employment of a Solution of Common Salt as an Eye Lotion in Ophthalmia. By Dr Isaac Hays. (*American Journal of the Medical Sciences.* August 1840.)—Dr Hays strongly recommends a saturated solution of common salt as a lotion in ophthalmia. He thinks that, in many cases of chronic granular ophthalmia, it has contributed more to the cure than any other application. He was led to its employment on the recommendation of an old woman, and has since then employed it in numerous cases with the most striking benefit. In some conditions of chronic granular ophthalmia, as where the eye is irritable, with injection of the conjunctiva of the ball, and lacrymation, he knows of no remedy which affords such prompt and marked relief.

On the Antisudorific Properties of Tannin. By M. Charvet. (*Bulletin Therapeutique,* July 1840.)—Sweating often goes on during the progress of disease to such a serious and dangerous extent, that the physician is obliged to prescribe particular remedies for its removal or alleviation. Many therapeutic agents have been recommended for this purpose, but all of them frequently fail; and even the acetate of lead, so highly recommended, though it possesses powerful antisudorific properties, is often highly objectionable from its liability to produce its peculiar poisonous effects when continued for any length of time. M. Charvet has been so fortunate as to discover that tannin, when administered in its pure state, is not only powerfully antisudorific, but liable to none of the objections urged against the other remedies given with the same view. He has extensively employed it in hospital practice for the last two years, and has met with very few cases which resisted its action. He has especially administered it in phthisical cases, where the hectic and night sweatings have been peculiarly troublesome; and he relates many cases where the tannin relieved this distressing symptom, though the patient was far beyond the reach of art.

M. Charvet administered the tannin in the form of pill, in the dose of from a third of a grain to a grain and three-fourths, with or without opium, as might be desired,—this medicine neither preventing or aiding its action. He always commenced with the smaller dose, and, as the system became habituated to the medicine, gradually increased it to the larger. He considered the evening as the most favourable time for its administration.

Case of Ascites cured by Bandaging. By M. L. Morelli. (*Annali Universali di Medicina,* March 1840.)—A woman, about 52 years of age, was seized with remittent fever with severe gastric symptoms, for the cure of which mercurials were freely administered, and produced copious salivation. Shortly after this symptoms of abdominal dropsy appeared, which she attributed to the mercurials she had taken. For years she had been treated for this affection, but remained unrelieved. She then entered the hospital, where the serous fluid was repeatedly drawn off, but again rapidly collected. At last M. Morelli, who had witnessed the good effects of bandaging the abdomen in the case of a little girl, resolved to try the effects of this mode of treatment.

A circular bandage, furnished with strong laces, and extending from the lower third of the sternum to the pubes, was put on, and pretty tightly laced; two bands were attached to its lower extremity and carried over the thighs, to retain it in its position. In a short time the good effects of the regulated compression became apparent. The size of the abdomen became much diminished, and at length assumed its usual size. The urine and other secretions became natural and regular; the appetite returned; and the person made a complete recovery.

Of Abdominal Tympanites. By Dr Schur. (*Casper's Wochenschrift fur die Gesammte Heilkunde.* July 1840.)—Dr Schur relates the history of two cases of tympanites which occurred in children at the breast. In both the abdomen was uniformly distended, and the gaseous fluid appeared to be contained in the peritoneal cavity, and not in the intestinal tube. In one of the cases a small incision was made into the abdominal cavity. Considerable hæmorrhage took place from the wound, much gaseous matter escaped, and the abdomen collapsed. The infant, though much weakened, recovered. The other child died, as the parents would not allow an incision to be made into the abdominal cavity. Dr Schur says that on dissection it was found that the gaseous fluid was contained in the peritoneal cavity, was inodorous, and did not come from the intestines, which were completely collapsed.

Dr Schur asserts that this form of tympanitis may be distinguished from meteorismus, where the gaseous fluid is contained in the intestinal tube, by the abdomen being uniformly distended, the navel very prominent, and no passage of wind by the natural passages.

SURGERY AND SURGICAL PATHOLOGY.

Case of Recovery from a severe injury of the Head. By Dr J. L. Burt of Cincinnati. (*American Journal of the Medical Sciences,* August 1840.) —A child, 3 years of age, fell from a window,' a height of about sixteen feet, and alighted on the crown of its head. When taken up, and for an hour afterwards, the cranial bones were so depressed as to present an almost level surface. Dr Burt describes its head "as flat as a table." The *os frontis* projected forwards at least two inches over the eyebrows, and presented all the symptoms of violent concussion of the brain.

The head was shaved, but, after a careful examination, no appearance of fracture could be discovered. After about an hour's interval, a spasmodic action of the facial muscles ensued, and shortly afterwards, the child vomited several times, which seemed to relieve it, and in a great degree to overcome the remaining stupor. Cold applications to the head, and enemata, which were speedily followed by copious evacuations, formed the treatment at this period. Sensibility returned after the bowels were opened. The cranium now began to assume its natural aspect, the flattened bones gradually rising to their position. The child rested well the first night; and the reaction was so moderate as not to require depletion in any form. By the end of the month the convalescence was established, and the bones of the head had returned to their usual form. The only treatment which had been requisite being cold applications to the head, and gentle cathartics.

Case of Enormous Empyema successfully treated by Operation. By Dr **Heyser.** (*Casper's Wochenschrift fur die Gesammte Heilkunde.* July 1840.)—A man, 22 years of age, was suddenly seized in the month of March 1839, with pain in his right side, for which he was bled and blistered, and had other appropriate treatment. His breathing, however, becoming more laborious, he entered the hospital on the 28th of April, when he presented the following symptoms: His respiration was very laborious; he spoke with difficulty and pain; he had frequent troublesome cough, with copious expectoration; had much anxiety; his face was livid; his pulse was small and very quick; the secretion of urine was almost stopped, but his bowels were pretty regular; he complained of feverishness and unquenchable thirst. The right side of the chest was an inch and a-half greater in circumference than the left, and gave a dull sound on percussion, and the respiratory sound there was inaudible. The right side of the chest was not elevated during inspiration, and there was considerable œdematous swelling all over that side. Whenever he attempted to lie on the left side he was threatened with suffocation.

On the 30th April, Dr Heyser, in presence of his colleagues, made an incision between the seventh and eighth ribs of the right side, when there escaped about twenty-two pounds Troy weight of inodorous purulent matter. The patient felt much relieved by the operation, and slept for a space of four hours after it. The wound was kept open by the insertion of a piece of oiled rag between its lips. Calomel and digitalis with decoction of quinine were administered internally.

The morning after the operation, between eight and nine ounces of purulent matter escaped from the wound, and for several days the patient continued in a very satisfactory state. After some time, however, the discharge became more abundant, and somewhat fetid, and at each dressing, which was repeated morning and evening, there escaped from five to eight ounces of fluid.

By the 3d of June the quantity of matter which escaped at each dressing had diminished to four drachms, and presented the characters of healthy purulent matter; he breathed freely, and had improved much in his general health. The lips of the wound were therefore allowed to come into contact, and speedily united. The right side of the chest, which before the evacuation of the liquid was an inch and a-half greater in circumference than the left, was now nearly one inch less than the left He could lie easily on both sides; his cough was almost gone; his respiration normal; a dull sound was elicited by percussion over the lower third of the chest, but sonorous elsewhere; the respiratory murmur, although still feeble, was distinct over the upper half of the chest. His pulse and general health were good, and at his own request, he was allowed to leave the hospital on the 9th of June.

He returned on the 16th of the same month, his uneasy symptoms having partially recurred; and upon opening the wound, and introducing a gum-elastic tube, about eight ounces of thin purulent matter escaped. Slight feverish symptoms followed, and the respiration was somewhat impeded. Sulphate of quinine and digitalis were again ordered, and, under the influence of these, the discharge of matter diminished, and the wound closed on the 14th of July. The other symptoms completely disappeared; and all the functions returned to their normal state. The dull sound on percussion, and absence of respiratory murmur at the lower part of the right lung still remained. He was dismissed on the 17th of July, and by the end of August was able to enter the military service.

On the Efficacy of Cod-liver Oil in Caries and Scrofula. By Dr **Taufflier** of Barr. (*Gazette Medicale de Paris,* October 1840.)—After some remarks on the folly of amputating in a scrofulous subject a limb for caries, which evidently only removes the local irritation, without curing the general state of the system upon which that local affection depends, Dr Tauffier strongly recommends the internal administration of cod-liver oil as a safe and efficacious means of remedying the faulty state of the system, which is the cause of the spontaneous occurrence of caries.

He relates four cases where both caries, white-swelling, and the scrofulous tendency appeared to be cured by the internal administration of that remedy; and he recommends the same means to be tried in every case of scrofulous affection, however desperate it may appear to be, as he is perfectly satisfied that these affections are curable by this remedy. The cases he relates are the following:—

A woman, 28 years of age, was affected with an advanced caries of the tibia of more than a year's standing. The limb had acquired an enormous volume, in consequence of the swelling of the bones and soft parts, which were traversed by six fistulous openings. She was then put on the use of cod-liver oil, and appropriate treatment was at the same time employed locally, and by the end of nine months a perfect cure was effected.

The second case was that of a little girl, 3 years of age, affected with a spontaneous caries of the bones of the tarsus, which was cured by means of the same remedy in the course of eight months.

The third patient was a cachectic man, 55 years of age, highly scrofulous, as well as all his family. He was labouring under extensive caries of all the bones of the tarsus, and of several bones of the metatarsus of the left foot. He was in such a pitiable state, and so worn out with his long sufferings and the profuse suppuration, that amputation was quite out of the question, even though it had been indicated. The cod liver oil was therefore administered to him, and under its use he amended slowly, but progressively. At present, Dr Taufflier considers him out of danger, and, as his general health is much improved, he has no doubt that a perfect cure will be obtained.

The fourth case was treated by Dr Brefeld. The subject was a scrofulous woman, 40 years of age, the index-finger of whose right hand had been amputated on account of a caries of the first joint. Caries, however, subsequently attacked the bones of the carpus and metacarpus, and in the course of a few years had involved a considerable number of these bones. She was then directed to take internally the cod-liver oil, which was followed after a considerable time with a notable improvement of the general health, and a complete cure of the caries.

On the Cure of Fissures of the Anus by Rhatany-Root. By Professor TROUSSEAU. (*Gazette Medicale de Paris*, 5th September 1840.)—M. Bretonneau appears to have been the first who recommended the rhatany-root for the cure of fissures of the anus. He was led to try its effect in this complaint from remarking, that constipation is in most cases the cause of the fissures, and the great obstacle to their cure. That this constipation is in a great majority of cases attended with a dilatation of that portion of the rectum immediately beyond the sphincter, which forms thus a place of lodgement for the feculent matters, which sometimes amass there to such an extent, as, when expelled, to cause pains equal in severity to those of delivery. To correct this morbid state of the gut, whether it was accompanied with fissures or not, and restore it to its original tonicity, was the object which M. Bretonneau had in view in employing the rhatany-root. In several cases, then, of this state of the gut, attended with fissures of the anus, he found, that he effected a cure both of the constipation and the fissures, by administering in glyster, the extract of rhatany-root, with the addition of a small quantity of the alcoholic tincture of the same.

Since the period when this mode of treatment was first made known, M. Trousseau has cured four cases, M. Marjolin one, and M. Berard, junior, two.

The mode in which M. Trousseau employs the rhatany-root is the following. He administers to his patient every morning a glyster of marsh mallows decoction, or simply of water, with the addition of olive or almond oil, in order to clear out the intestines. Half an hour after the intestine has been emptied, he gives an injection composed of thirty-eight drachms of water; one to two drachms and a half of the extract of rhatany; and five drachms and a half of alcohol, which the patient is desired to retain, if possible. The same styptic injection is repeated in the evening.

When the pain is once moderated, only one glyster is given daily; and when the cure appears to be completed, every alternate day only, for a fortnight longer.

He says he has derived considerable advantage from the employment of an ointment composed of one or two parts of the extract of rhatany to five of the butter of cacao.

Successful Case of Cure of General Articular Deformity by the subcutaneous section of Forty-two Muscles, Ligaments, and Tendons. By Dr Jules Guerin. (*Gazette Medicale de Paris*, 5 September 1840.)—A young man, about 22 years of age, who was labouring under general articular contractions, caused by the active retraction of certain of the muscles and ligaments, and rendered more permanent in consequence of an affection of the brain, was operated on by Dr Jules Guerin on the 25th of August of the present year. Forty-two muscles, ligaments, or tendons were cut through the same day, but only twenty-eight minute apertures were made through the skin, and the operation lasted about an hour. These sections were made in the way recommended by Dr Guerin for all such operations, viz. by making a small incision through the skin at some little distance from the spot selected for the section of the muscle or tendon, and so small and oblique as to prevent any access of air to the internal wound.

The muscles or tendons cut through were the following. The *pectoralis major* of one side. At the elbow-joint of both arms there were cut through the *biceps flexor cubiti*, the *pronator teres*, the *flexor carpi radialis*, the *flexor digitorum sublimis*, and the *palmaris longus*. On the fore-arms were divided the tendons of the *extensores carpi radialis longior* and *brevior*, those of the palmar muscles, and of the two extensors of the thumb. At the knee-joint of both legs, there were divided the *sartorius*, the *biceps flexor cruris*, the *semi-membranosus*, the *semi-tendinosus*, and the *gracilis* muscles, part of the *fascia lata*, and the external lateral ligaments. At both feet were incised the *tendo Achillis*, the *extensor proprius pollicis pedis*, the *extensor digitorum communis*, the *tibialis anticus*, and the *peroneus* muscles.

The patient complained little of the pain of these various operations, and was not much fatigued; and an hour after them fell into a calm and refreshing sleep. The night and day following he passed tranquilly. No inflammatory action was excited, and the twenty-eight small incisions were perfectly healed by the third day after the operation; and when this account was published the traces of the cicatrices could scarcely be observed.

The operation was performed in the presence of a number of very distinguished medical men, whose names are published in the account.

Case of Amputation at the Shoulder-joint with removal of the Scapula. By M. Renoult. (*Bulletin de l'Academie Royale de Medecine*, 30th July 1840.)—M. Renoult reported to the Academy the following interesting case, operated on by Gaetani-Bey, the celebrated Egyptian surgeon. An Egyptian workman, employed in the founderies, had his shoulder severely injured, and one of his testicles blown off, in consequence of an explosion. The humerus was shattered, as well as the scapula, and the external extremity of the clavicle. The soft parts were also severely lacerated. Amputation at the shoulder-joint was immediately performed; the fragments of the scapula extracted; and the shattered extremity of the clavicle removed. The wound was then closed as far as the injured state of the soft parts would allow, and, in spite of the serious nature of the operation, the patient made a complete recovery, and left the hospital fifty-five days after the accident.

TOXICOLOGY.

Poisoning by the Vapours of Antimony. By M. Lohmeier. (*Gazette Medicale de Paris.* September 1840.)—M. Lohmeier, during the summer of 1839, had occasion to attend four patients, whose symptoms could not be referred to any known disease. All the four were frequently exposed to the vapours of antimony, in an establishment where there were prepared on the large scale tartrate of antimony, butter and glass of antimony, and other antimonial preparations, during the preparation of which were disengaged abundant vapours of antimonious and antimonic acid, and chloride of antimony.

The four patients presented the following symptoms. Pain in the head; lancinating pain along the edge of the ribs and in the back; difficult respiration with mucous and sibilous rattles over the chest; difficult expectoration of tenacious mucus; sleeplessness, anorexia, diarrhœa, profuse perspirations, and general weakness; dysuria with a mucous discharge, causing a burning feeling in the urethra; flaccidity of the penis, with loss of the sexual appetite, and even complete impotence; pain in the testicles, and atrophy of these organs, as well as of the penis; pustules over different parts of the body, but especially on the thighs and scrotum.

M. Lohmeier observes, that in none of the recorded cases of poisoning by antimony has the peculiar affection of this medicine on the sexual organs been observed, nor yet its effect in exciting a peculiar cutaneous eruption; but it is remarked that the homœopathists have noticed this effect of antimony on the generative organs, and that its preparations are habitually administered to animals by agriculturists to hasten their fattening, when it probably acts by diminishing the sexual appetite; and lastly, that it appears to have been used by monks in the monasteries with the view of diminishing their sexual propensities, and hence its name of antimony, from *moine*, a monk.

The curative means used by M. Lohmeier were the free use of antiphlogistics in the first place, and afterwards the internal administration of opium, tannin, and quinine, with lotions of the same applied to the parts externally affected, and milk diet. If the functions of the sexual organs did not return naturally as the other affections abated, he recommended the use of tincture of cantharides united with opiates, and lotions of cold water to the scrotum.

Lemon-Juice the antidote for poisoning by Hyoscyamus. By M. Medoro. (*Giornale per service di progressi della Pathologia et della Therapeutica*, Feb. 1840.)—Dr Medoro relates four cases where the poisonous symptoms resulting from an overdose of hyoscyamus were instantly *arrested by the* administration of lemon-juice.

The first case occurred in the person of an aged individual, who had been operated on for the relief of strangulated hernia. Three grains of the extract of hyoscyamus were administered to him for the purpose of inducing sleep and calming his agitation, but far from producing any sedative effect, this dose produced a kind of mental alienation. The medicine had been given in the evening, and by the middle of the night he was found in a state of violent delirium. His face was much flushed; his eyes fixed and glaring; and the muscles of the face and of the lower jaw agitated by convulsive movements. He recognized no one, spat in the face of those who approached him, and endeavoured to kick them. He was obliged to be kept in bed by force. He was forced to swallow the juice of one lemon, and the symptoms described above disappeared as if by enchantment.

The second case occurred in a neuralgic patient, who, after a long course of treatment, was at last put on the use of extract of hyoscyamus. For two days he took daily thirteen and one-third grains of the extract of hyoscyamus, without experiencing any inconvenience; but on the third day the same symptoms which occurred in the above case came on, but not with the same severity. There were symptoms of mental alienation; spasmodic contractions of the muscles of the face and of the lower jaw; *subsultus tendinum*; and a very rapid pulse. A large dose of lemon juice quickly dispelled these unpleasant symptoms, and restored the patient to his former state.

The third case occurred in a strong man who had suffered a comminuted fracture of the leg, and in whom *risus sardonicus* and muttering delirium were produced by taking about nine grains of the extract in two pills.

The fourth case occured in a young woman, recently delivered, who had the same symptoms produced by half a grain of the extract given in an emulsion. In both these last cases the juice of lemons produced a very rapid removal of the symptoms of poisoning; and Dr Medoro thinks that he is justified in arriving at the conclusion, that lemon juice is the antidote to hyoscyamus, and perhaps to others of the narcotic vegetables.

Substance employed in 194 *cases of Poisoning in Paris, from* 1832 *to* 1838. (*Annales d'Hygiène Publique*, October 1840.)

Metallic arsenic or fly powder,	5	Brought over,	159
Arsenious acid,	132	White lead,	2
Sulphuric acid,	5	Preparations of copper,	13
Hydrochloric acid,	1	Tartar emetic,	1
Nitric acid,	2	Morphia,	1
Prussic acid,	1	Nux-vomica,	1
Ammonia,	1	Opium,	3
Arseniate of copper,	1	Bichloride of mercury,	10
Belladonna,	1	Sulphate of zinc,	1
Cantharides,	10	Artificial sulphate of arsenic,	3
	159	Total,	194

OBSTETRICS.

On the Cure of Procidentia Uteri by Suture of the External Labia. By Professor E. Geddings. (*American Journal of the Medical Sciences.* August 1840.) Dr Fricke of Hamburg appears to have been the first who endeavoured to unite the external labia for the purpose of retaining the prolapsed uterus in its situation. This operation, to which the name *Episioraphy* has been given, appears to be preferable to several of those which have of late been suggested and employed for the relief of that malady. Should this operation fail, it may be repeated, and it possesses this most important advantage, that it can be performed at any period of life, as it does not interfere with menstruation, conception, or child-bearing.

Professor Geddings has operated successfully in four cases; and the following is the manner in which the operation was performed. The patient was placed in the ordinary position for lithotomy, and the prolapsus reduced. One labium was then put on the stretch by an assistant, and an incision was commenced with a common scalpel, about a finger's-breadth from the upper commissure, and the same distance from the edge of the labium. The incision was carried downwards with a bold sweep, and terminated by a slight curve inwards, and at a little distance behind the fourchette. A slip of the labium of a finger's-breadth in thickness was thus severed from the external parts, taking care not to cut through the mucous membrane of the vagina. Making traction on this slip, downwards and inwards, the mucous membrane of the lateral portion of the vagina was then dissected up to the extent of an inch and a half, and detached with the excised labium. The same thing was repeated on the opposite side, the incision being so directed as to intersect the first cut at an acute angle, and remove the fourchette with the other parts. After the hæmorrhage, which was generally inconsiderable, had ceased, an oiled sponge was passed into the vagina, and the two raw surfaces brought into apposition by means of the quilled suture of five stitches. The parts were dressed with a compress of soft lint, secured by a T bandage, and the patient put to bed, with her ankles and knees properly secured together by means of bandages. It was not found necessary to use the catheter, but considerable advantage was derived from keeping the dressings constantly wet with cold water. The introduction of the sponge is for the purpose of preventing the descent of the uterus till adhesion has taken place, and was generally removed about the fifth or sixth day. The sutures were removed in proportion as the parts appeared to be united.

In the four cases operated on by Professor Geddings, no return of the complaint had occurred at the period of the publication of his paper, and they had all returned to their usual avocations.

On the danger of injecting Fluids into the Uterus. By M. HOURMANN. (*Journal des Connaissances Medico-Chirurgicales*, July 1840.)—M. Vidal recently recommended the employment of injections into the cavity of the uterus for the cure of various affections of that organ, and stated that he had found their use very efficacious, and never followed by any unpleasant symptoms. Bretonneau, Ricord, and others who had made use of this practice, had, however, arrived at a very different conclusion, as they found these injections liable to produce many disagreeable effects, acute peritoneal inflammation, and death.

M. Hourmann relates a case where violent abdominal pain, followed by metro-peritonitis, was caused by means of injecting a decoction of walnut leaves into the uterus, for the cure of an obstinate leucorrhœal discharge, which had been traced to come from the cavity of that organ. The patient recovered in consequence of a violent attack of hemorrhage from the uterus, occurring most opportunely within forty-eight hours after the administration of the injection.

M. Hourmann, wishing to ascertain whether these dangerous symptoms could be produced from a portion of the fluid having passed through the Fallopian tubes into the cavity of the abdomen, found, on injecting fluid into the uterus after death, that such was actually the case. M. Nelaton, in repeating the same experiments, observed a still more *extraordinary* circumstance. The liquid injected into the uterus of a woman who had died of an attack of erysipelas of the face, did not penetrate into the Fallopian tubes, but distended one of the veins of the broad ligament of the uterus, pushing before it bells of air.

Delivery of Four Children at a Birth. By M. Bourdois. (*Journal des Connaissances Medico-Chirurgicales*, July 1840.)—A woman who had been married for twenty-three months, was, on the seventh month of her pregnancy, delivered of a male child. Two hours afterwards a second male child was born, then a third, and after a few minutes a fourth, all of the male sex. A fresh discharge of *liquor amnii* took place before the delivery of the second child. There were, in fact, two deliveries, or a double pregnancy. One placenta had but one cord attached to it, the other had three cords. As it was necessary to introduce the hand into the uterus for their extraction, they were found to be attached to opposite sides of the uterus. The two first born and the last were apparently of equal strength, and had the look of seven months' fœtuses. The third born was much less perfectly developed, and had more the appearance of a five months' fœtus. The third born lived but a few seconds, the three others only a few hours.

Case in which Milk was expectorated after each delivery. By M. Demetrio Rasi. (*Bulletino del Scienze Mediche di Bologna*, April 1839.)—A lady born of parents who lived to a great age, married at the early age of 15. She was prevented from nursing her first child, in consequence of suffering from an attack of pneumonia; but nothing particular was observed with regard to her breasts. During her second pregnancy, she was frequently bled to relieve severe fits of asthma, but nevertheless, was delivered of a healthy stout child. A violent attack of fever seized her shortly after delivery, and the breast swelled and became much hardened. An abundant secretion of milk took place in the breasts, but neither suction by means of the child nor by instruments could draw a drop of milk from the nipple. Strong purgatives were therefore administered, with the view of stopping the secretion of milk, and hot fomentations were applied to the breasts. On the third day of this treatment the feverish symptoms began to abate, and at the same time a short, dry, frequent cough made its appearance. The cough soon became more free, and was attended with expectoration of simple mucous fluid, which gradually increased in quantity, and assumed all the physical qualities of true milk. This expectoration of milk continued for fifteen days; during which time the patient expectorated in general about 90 drachms of milky fluid. By the fifteenth day the breasts had resumed their natural appearance.

The same phenomena were observed in six consecutive deliveries in this lady: but in her last she was attacked with acute phthisical symptoms, and died within fifteen days after delivery.

MEDICAL POLICE.

Official Document as to the Re-vaccinations over France. By M. Villeneuve. (*Annales d'Hygiene Publique*, October 1840.)—M. Villeneuve has published a most interesting table, constructed from the official documents of the vaccinations, re-vaccinations, and cases of small-pox after vaccination, which have occurred over 41 departments of France. It appears unnecessary to give in detail the particulars of each department. The general result is as follows:

30,413 individuals were vaccinated; 29,853 of these proved successful, and 560 failed. 2,199 cases, after a longer or shorter period, were *attempted to* be re-vaccinated, but only 223 cases succeeded in taking the vaccine disease a second time, whilst 1,976 cases failed. 365 cases of small-pox occurred after vaccination; but of this number only 6 died.

On Infanticide in France. By Dr H. **Bayard.** (*Annales d'Hygiène Publique*, October 1840.)—The crime of infanticide appears to be increasing in France to a fearful extent, as is shown by Dr Bayard in his present paper. He gives several short tables, which demonstrate that, for the last nine years, the crime has been progressively increasing in frequency. Thus, in the year 1830, the number of individuals accused of, or implicated in the crime of infanticide was 287, but by 1838, the number had increased to 465; and the total number for the nine years amounted to no fewer than 3759 individuals. During the same period of time, there occurred 564 homicides of new-born infants by the imprudence of their mothers; and if these be included in the numbers of infanticides, it appears that no fewer than 4323 individuals have been accused of, or implicated in, the crime of infanticide in France during the above-mentioned period of nine years.

Statistics of the Bloomingdale Asylum for the Insane. By Dr **James Macdonald.** (*New York Journal of Medicine and Surgery*, October 1839.)—The Bloomingdale Asylum near New York is one of the largest of the hospitals for the insane which exists in the United States. It was opened in the year 1821, and from that period, up to 1836, no fewer than 2037 patients had been admitted into it. The average number of patients annually admitted for that period was 130, the numbers being tolerably constant, excepting during periods of great commercial distress, viz. the years 1825 and 1826, when the numbers were raised to 156 and 142, and the prosperous year of 1829, when the number was reduced to 90.

Of 1680 cases admitted from 1824 to 1836, the following was the particular forms of insanity under which they laboured. Mania, 770; monomania, 386; dementia, 226; idiocy, 7; delirium a potu, 291.

The seasons appeared to have considerable influence on the number of admissions. Thus, for a period of twelve years, the fewest admissions took place in January, when only 70 were admitted, but there was a regular rise in the scale of admissions till June was reached, when they attained a maximum of 173; after which period they maintained an uninterrupted descent, month after month, till January.

The admissions, as relating to the state of life, were 600 married individuals; 573 unmarried persons; and 84 widows or widowers. 1346 belonged to the male sex, and 691 to the female.

It appears from the tables which Dr Macdonald gives, that the classes of persons most subject to insanity are those which are connected directly or indirectly with commerce. Thus, of 804 persons whose occupations were ascertained, 242, or 30 per cent., were dependent on commerce for subsistence.

The table of the causes of insanity is particularly interesting, and is as follows.

1. *Physical Causes.*

Hereditary, . .	155	Congenital, . .	13
Cerebral disease, .	55	Rapid growth at puberty,	13
Bodily disorder, .	48	Metastasis, . .	9
Following parturition, .	43	Cessation of menses, .	8
Constitutional, . .	34	Insolation, . ,	6
Succeeding fever, .	31	Eruptive fever, . .	6
Disease of uterus, .	20	Suppression of discharges,	5
Masturbation, . .	16	Poison of lead in cyder or in fumes, . . .	5
Injury of head, .	15		

Connected with lactation,	4
Repelled eruptions,	4
Syphilis and mercury,	3
Old age,	2
Spinal disease,	2
Pregnancy,	2
Sedentary habits,	2
Excessive use of tobacco,	2
Hygiene pills,	2
Suddenly checked perspiration,	2
Change of climate and habits,	2
Excess in venery,	1
Inhalation of prussic acid,	1
Total of physical causes,	511

2. *Moral Causes.*

Pecuniary embarrassments,	59
Religious excitement,	40
Domestic trouble,	31
Over-exertion of mind,	26
Grief for loss of relations,	23
Unrequited love,	20
Unhappy marriage,	17
Anxiety of mind,	17
Wounded pride, disappointed ambition,	13
Religious anxiety, doubt, despondency,	12
Remorse,	13
Terror,	9
Avarice,	8
Religious terror,	7
Defective moral education,	6
Sudden reverse of fortune,	3
Sudden prosperity,	3
Indulgence of violent temper,	3
Excessive novel reading,	3
Want of employment,	4
Jealousy,	2
Loss of office,	2
Indulgence in speculation,	2
Political excitement,	2
Anti-masonic excitement,	2
Ungratified desire to return to native land,	1
Excess of joy,	1
Desertion after seduction,	1
Indulgence in theatrical amusements,	1
Total of moral causes,	332
Total of physical and moral causes,	843

The proportion of recoveries in this asylum was as follows:

From 1821 to 1836 inclusive, 2037 patients were admitted; of this number, 898 were dismissed cured; 355 improved; 437 by request; 9 as improper objects; 39 made their escape; and 160 died. This gives a proportion of 44 per cent. of recoveries during that period. The per centage of the recoveries amongst the males was greater than amongst the females. Thus, of 1346 males admitted, 649 recovered, or 48¼ per cent.; whilst of 691 females admitted, only 249 recovered, or 36 per cent.

Cases of old standing were found to be much less curable than recent cases. Thus of 750 old cases, only 51 recovered, or rather less than 7 per cent. Whilst of 925 recent cases, no fewer than 708 cases recovered, or the large proportion of 76½ per cent.

From 1825 to 1836, classing the recoveries according to the season of the year, they stand thus: winter months, 136 recoveries; spring months, 167: summer months, 189; autumn months, 218.

Report by the Royal College of Surgeons of Edinburgh, on the Proposals of Messrs Warburton and Hawes for the amendment of the Laws relating to the Medical Profession in Great Britain and Ireland; adopted unanimously, 12th December 1840.—The Royal College of Surgeons of Edinburgh, having been unsuccessful in a series of attempts, during a period of more than twenty years, to obtain from Parliament a legislative remedy, founded upon broad and general grounds of public advantage, for the inequalities of education and of privilege which affect the medical profession in this country, rejoice to find, that there is at length a prospect of this subject being brought fairly under the consideration of the Legislature, with such an amount of support from the profession and from the public as to insure the attainment of some measure of practical good.

The progressive improvement in the courses of study prescribed, and in the minuteness and accuracy of the examinations instituted, by the different Boards to which the power of licensing is at present committed—an improvement in which the College have at all times taken a very prominent and leading part—has hitherto constituted the chief security to the public for the possession of a well-educated body of medical practitioners, and for the advancement of medical science itself. Under this system, there has risen up in Great Britain and Ireland a large body of highly instructed medical practitioners, who feel aggrieved by the disabilities and inequalities of privilege to which they are subjected; who are conscious that their importance in the community entitles them to a remedy; and who are, with good reason, determined to urge their claim to that remedy as being due, not merely to their own interests, but to those of the public at large.

Some years ago, such a degree of improvement in the system of granting medical qualifications by means of the existing incorporated bodies, as would have secured to the holders of those qualifications equal education and equal privileges throughout the united kingdom, was all that the most sanguine medical reformers desired to effect. But an anxiety has recently been manifested, and very loudly and generally expressed, to accomplish those and similar objects by means of a general representation of the medical practitioners of this country. While the College have not hitherto given any encouragement to this project, they do not view it with any particular apprehension; and seeing how much importance is attached to it by professional men of the highest character, they are willing to hope that a representative body, chosen by the practitioners of the healing art, if properly arranged, and accompanied by effectual checks for securing the community against the influence of partial views and interests, might work beneficially both for the profession and the public.

It is a gratifying proof of the just appreciation of the importance of this subject by those who are not of the Medical Profession, that two Members of the House of Commons, Mr Warburton and Mr Hawes, have each promulgated a plan of medical reform, and that their several proposals evince an enlightened acquaintance with the nature of the evils to be remedied, which could only have been acquired by a great sacrifice of time and labour. In both of these proposals are contained many valuable suggestions; and in both, as might have been expected, there are matters which seem to the College to require re-consideration.

The subject of Medical Legislation may be considered as it respects the interests of *the Public*, those of *Medical Practitioners*, and those of *Medical Incorporations*.

I. The *Interests of the Public* call for the following provisions:

1. That all persons who receive a legal recognition as practitioners of medicine, should be previously subjected to a uniform and efficient system of education and examination.

2. That the public should have an opportunity, by means of a system of registration, of distinguishing those who have received this public recognition from those who have not.

3. That the public should be permitted to select their Medical Attendants, from among the number of those who are thus recognized unembarrassed by any exclusive privileges of particular classes of medical practitioners.

II. When the subject is considered with a more immediate reference to *the interests of Medical Practitioners*, all of these conclusions appear to be confirmed. The following expressions of Mr Warburton on this subject appear to the College to be just and apposite.

"The legitimate means of discouraging unqualified practice appear to me to be the following:

"1. Uniformity of system in respect of medical instruction and examinations, and uniformity of professional privileges founded thereon; so that the public may see and know that when a man has passed his examination, the State considers him a fit person to practise *ubicunque gentium.*

"2. Registration of all who practise, distinguishing the qualified from the unqualified, that the public may be informed by the State what practitioners are to be confided in.

"3. Employment by the State, and by all public institutions, of none but the qualified.

"4. Punishment, as for fraud, of all persons who practise, assuming a professional title which does not belong to them." *(See Mr W.'s Letter to Dr Maunsell in the Dublin Medical Press,* No. xcvi., p. 300.)

Besides these four methods of protecting the qualified practitioner, and discouraging the unqualified, another has been much agitated by medical reformers, viz. the establishment of a system of penalties against the latter, for practising medicine without a license. In regard to this, Mr Warburton says in another letter (*Dublin Medical Press,* No. xcvi., p. 301,) "I have no faith in the efficacy of heavy penalties, when sought to be enforced against obscure people." This seems a sound opinion: it is conformable to the actual practice of this College, which has for a long period desisted from prosecuting those who have infringed its exclusive privileges, and has had no cause to regret its abstinence from this practice; there being no town in which ignorant pretenders to medical skill are less numerous and less respected than in Edinburgh. There is much reason to fear that prosecutions will be found to fail in accomplishing the object proposed. They may render the unqualified more cautious in practising on public credulity, without making them less successful; they may even impart to their pretensions a degree of fame and of importance, which those pretensions would never acquire without such assistance; and they may have an unfavourable impression on the character of the qualified, who will be more likely to stand well with the unprofessional part of the public, if they have the magnanimity to despise this species of artificial protection, and to rest their claims to public confidence solely upon their professional talents and qualifications. There cannot, of course, be any objection to their endeavouring to strengthen these claims by acquiring those honorary distinctions which universities and other public bodies are in the practice of conferring.

III. But the subject of a reform in the laws affecting the profession must also be considered in its bearings on *the interests af the existing Medical Incorporations.* These incorporations have been objected to on the following grounds: 1. Their exclusive rights of examining and licensing the practitioners in certain districts, which have the effect of disqualifying from practising within those limits, well educated men who have obtained a qualification elsewhere; 2. The disparity in the conditions required of those who are admitted to examination by the different boards and universities, both as regards the length of the period of study, and as regards the number of branches of instruction prescribed by their several curricula; and, 3. The very different degrees of rigour with which the examinations for licenses or degrees are conducted by the different Boards. The College have no hesitation in expressing an opinion which they have long entertained, and often promulgated, that the exclusive rights and restrictions referred to under the first of these heads, are altogether unsuited to the present state of the profession. The only justification of them which has ever been proposed is based upon the disparities specified under the second and third heads. Great irregularities in regard to education and examination undoubtedly exist, and constitute the plea for the obnoxious inequalities of privilege; and it will certainly be expected of medical reformers, that when they propose to abolish this last species of inequality, they will at the same time be prepared to admit the necessity of taking effectual means for the abolition of the two former also. About inequality of education, no great difficulty would now be experienced. An imperative system, sanctioned by some public authority, could easily be introduced and made applicable to every part of the united kingdom. The Colleges of Surgeons of London, Edinburgh, and Dublin, in a conference which they held in London by delegates, during the discussion in Parliament of the Irish Charities' Bill, came to an understanding as to what should constitute the *minimum* of study, without evidence of having gone through which no one should be admitted to examination by any board. The joint resolutions of these bodies were afterwards acceded to by the Faculty of Physicians and Surgeons of Glasgow. A similar scheme was resolved upon by the University of Edinburgh, and the Colleges of Physicians and Surgeons of Edinburgh. By a comparison of these plans, and of those of other licensing boards, a uniform system of education could without any serious difficulty be adjusted.

But uniformity in the degree of rigour and minuteness in conducting examinations is by no means so easy of attainment, and indeed, nothing better than an approximation to such uniformity can be expected. The object is undoubtedly of the greatest importance to the interests of the public, and no scheme of professional reform would deserve the name, if it did not attempt to secure as great a degree of uniformity in examination as can be attained. There are only three methods which occur to the College by which this object may be accomplished. The *first* of these is the plan proposed by Mr Warburton, and consists in intrusting the business of examining and conferring professional qualifications to a single board, appointed by a medical council in each of the three divisions of the kingdom. It is further provided, that the examinations shall be conducted according to a set of bye-laws enacted by a Medical Senate for the whole kingdom, and shall be under the immediate inspection of the Councils and of the Senate. The College cannot but feel that the rights of the existing Boards to grant licenses applicable to their respective bounds, though proposed to be retained under this scheme, would be virtually abrogated, since no one would waste his means in acquiring the minor privilege, when the more extensive one applicable to the whole of the British dominions was made accessible. This proposal, therefore, if carried into effect, would altogether supersede, and indeed destroy, a number of establishments created at great cost to the public and the profession, and containing within them most if not all of the elements requisite for the successful performance of the functions hitherto intrusted to them; establishments, of some of which it may safely be averred that they have discharged their functions with the greatest liberality and disinterestedness. To destroy the powers of these boards, and thus to cut off from the incorporations appointing them those funds which are at present applied to purposes of great public utility, would be a gratuitous injury to the public interests, and the attempt would, in all probability, encounter such opposition as would be fatal to it in Parliament. The College readily acknowledge that if the laudable object which Mr Warburton seeks to accomplish could only be attained by means of this proposal, the privileges of the incorporations, conferred solely for the public weal, could never be permitted to stand in the way of it. But the College are of opinion, that either of the other two methods yet to be commented on would be preferable as a means to the proposed end.

The *second* plan (*see below, Comparative Statement*, No. 9) for assimilating examinations is that of Mr Hawes, which, like the former, provides a *Senate* for the whole kingdom, with power to make bye-laws for regulating education and examinations; and a Medical *Council* in each of the three divisions of the kingdom, with power to select a board of examiners for carrying these laws into effect under the superintendence of the Councils and Senate. Thus far there is a coincidence between the two plans. But there is this very important difference, that Mr Hawes's plan provides, that the bye-laws "shall not be good and valid unless they require that, previous to the final examination of any person desirous of obtaining a diploma of qualification to practise the Art of Medicine, he shall produce a diploma, certificate, or letters-testimonial of having taken a degree in Medicine, or of having passed an examination in Medicine or Surgery before some University, College, Hall, or other persons legally entitled to grant a diploma, certificate, or letters-testimonial at the time of the passing of the act." This proposal secures the continued existence of the present machinery for granting professional qualifications, and avails itself of that machinery by making it imperative for a first stage of examination, but makes a second trial necessary before the Board appointed by the Council. This examination, which is obviously intended to operate as a check on any laxity on the part of the existing Boards, and to equalize their proceedings, would certainly effect these objects to some extent, but by a procedure which is open to some objections.

The College conceive that a *third* scheme might, without much difficulty, be devised, which, leaving the immediate conduct of the examinations in the hands of the existing Medical Boards, would, at the same time, place them under the superintendence of the Senate or Councils proposed to be instituted, acting through the intervention of Assessors specially appointed by them for the purpose. Such a conjunction of local examinations with general superintendence would possess the twofold advantage of avoiding any infringement of the privileges of existing Institutions, and of, at the same time, removing all apprehension or suspicion of the examinations being conducted inefficiently by any Board, or with less strictness by one Board than by another. To many of the members of the College it appears that this conjoint method of testing the candidates' qualifications, by examiners appointed by the existing institutions, and assessors nominated by the Senate or Councils, might be rendered at once more easy of execution and more efficient, by taking advantage of the system of examining by written questions, now pursued in many institutions, in judging of the claims of candidates to degrees and other honours.

The examinations might either embrace all the subjects in the curriculum at one sitting, or separate sittings might be given to the departments of Medicine, Surgery, and Midwifery; and it would be necessary that those Boards which do not examine on all the subjects in the curriculum should make an arrangement for co-operating with other Boards, in the same place, which might examine on those subjects to which their own examinations do not extend; so that every part of the course of study might be included in the examinations. The fees payable to these Boards should be uniform and moderate; and the proceeds, after defraying the expenses of the examinations on a scale approved of by the Councils, should be applied, as they have for many years been applied, by this College, to purposes of public and professional utility.

In concluding these general remarks on the subject of the proposed *reform, as it* respects the interests of the Public, of *Medical Practition-*

ers, and of the *existing Medical Institutions*, the College is fully aware that when these three views are contemplated with the eye of an enlightened liberality, they must be found to be coincident. The College can aver, with truth, that they have pursued no corporate ends to the public injury; that they have sought to raise the standard of medical acquirement in this school by an enlarged system of education, proportioned to the wants of the age; that they have refrained for a very long term of years from making additions to the cost of their diploma; that their examiners have submitted to be greatly underpaid in order to enable the College to maintain their ground without such additions; and that their entire accumulations have been expended in erecting their Hall, and purchasing and maintaining a Museum, to the accommodation of which the greater part of the building is devoted, and which is accessible to the public, and most beneficial to the Medical School. Having made such use of their present privileges, the College conceive themselves entitled to claim the credit of disinterestedness in any advices which they may offer in reference to Medical Legislation; and they do most fully and unequivocally admit the supremacy of the public interest in all that relates to this most important subject. Let the medical incorporations be compelled to submit to all such regulations as the welfare of the public requires; let their privileges, in particular, altogether cease to be exclusive and local in their character; but let not an apparatus, so capable of being beneficially employed, be gratuitously and inconsiderately annihilated.

The College now proceed to the examination of the two proposals before the public, those of Mr Warburton and Mr Hawes; in commenting on which they will follow the order of the clauses of the latter, as being in some respects the more convenient one for the purposes of commentary. These proposals will be rendered more intelligible by the following tabular view of the several provisions:—

Comparative Statement of the principal enactments for regulating the Medical Profession, contained in a Bill brought into Parliament during last Session by Mr Warburton, and in a draft of a Bill proposed to be brought into Parliament next Session, by Mr Hawes.

Bill of Mr Warburton.	*Draft by Mr Hawes.*
1. All persons practising medicine to be registered, whether possessed of a "medical qualification" or not; but in separate and distinct lists. The names of practitioners having a medical qualification to be published annually; those of the practitioners not having one, "in any or in every year," according to the discretion of the Principal Secretary of State for the Home Department.—Clause 3, c. 1, c. 9, c. 10.	1. All qualified practitioners, but no others, to be registered, and their names to be published annually.—c. 5.
2. A Council of thirty-six members to be formed in each division of the kingdom; twelve councillors to be appointed by the Crown, being persons "who neither are, nor ever have been, engaged in the practice of medicine;" and twenty-four qualified practitioners to be elected as councillors by the registered qualified practitioners of that division.—c. 19.	2. A council to be formed in each division of the kingdom, consisting of twenty members, (not specified whether to be medical men or not), to be chosen by the licensed medical practitioners of the division, and one to be added by each of the medical incorporations of the same division.—c. 6.
Provision is made for an annual change of a fourth part of the elected councillors, the persons to vacate to be suggested by	The whole of the councillors to be elected triennially.

Bill of Mr Warburton.	*Draft by Mr Hawes.*
the council, and to be determined by the electors.—c. 20.	
3. A Medical Senate of the United Kingdom to be formed, consisting of thirty-six members, twelve nominated by each of the National Councils, viz. four of the non-medical men who had been appointed to the council by the Crown, and eight of the medical councillors.—c. 22.	3. A senate to be formed of nine members, (not specified whether to be medical men or not), three to be chosen by each of the national councils —c. 21.
All the senators to go out of office annually.	The senators to remain in office for "five whole years."
The senate to be entrusted with the making of bye-laws for regulating medidical education and examinations, schools of medicine, &c.—c. 31.	The senate to be entrusted with the making of bye-laws for the education and examination of candidates for the license to practise, &c.—c. 28.
4. The bye-laws made by the senate "to be presented annually to both Houses of Parliament."—c. 24.	4. Power given to Her Majesty's Privy-Council to disallow any of the bye-laws made by the senate.—c. 28.
5. A College of Medicine of the United Kingdom to be formed, admission to the fellowship of which may be obtained in one of three ways, viz. 1. By election as councillor; 2. By ballot of the council as afterwards specified; and 3. By undergoing examination by examiners appointed by any of the councils.—c. 29, c. 30, c. 31.	5. All practitioners holding medical qualifications at the time of the passing of the act, and all persons licensed under the regulations of the senate, to obtain, by the simple act of registration, all privileges.
6. "Fellows of the College of Medicine of the United Kingdom" to be entitled to practise as surgeon-apothecaries, or general practitioners of medicine, in any part whatsoever of the British dominions.—c. 35.	6. No persons to be allowed to practise in any part of the kingdom but those who have obtained licenses from one of the national councils as "medical practitioners," and these are to be entitled to practise all the branches of the profession in any part of the British dominions.—c. 30 and 32.
Licentiates of any of the existing colleges, not being fellows of the new college, to be limited to the bounds over which their present privileges extend.—c. 58.	
7. Fellows of the College of Medicine of the United Kingdom to be authorized, in addition to the privileges they enjoy as fellows, to practise as physicians in any part of the British dominions, and to act as physicians in public institutions, provided they be graduates in medicine or physic in any university in the united kingdom, and have undergone an additional special examination on the art and science of medicine before the examiners of one of the national councils, subsequently to their having obtained the fellowship.—c. 36.	7. Reference is made to only one class of practitioners, educated and examined in all the departments of medicine, and licensed to practise all the branches, in all situations, and in all parts, of the British dominions.—c. 37.
Fellows of the college to be authorized, in addition to the privileges they enjoy as fellows, to practise as surgeons in any part of the British dominions, and to act as surgeons in public institutions, provided they be fellows, members, or licentiates of some college of surgery, &c. in the united kingdom, and have undergone an additional special examination in surgery, before the examiners of one of the national councils, subsequently to	

Bill of Mr Warburton.	*Draft by Mr Hawes.*
their having obtained the fellowship of the College of Medicine of the United Kingdom.—c. 37. Similar provisions are made respecting accoucheurs, apothecaries, &c.—c. 38, 39, 40.	
8. Admission of practitioners, holding a medical qualification, at the time of the act coming into operation, to the Fellowship of the College of the United Kingdom, to be determined by the ballot of one of the councils, under bye-laws to be framed by the senate, relatively to the particular qualification or qualifications which they may hold.—c. 30.	8. A certificate to practise the art of medicine to be granted by the registrars to every person who shall apply for it, on producing a diploma, certificate, or license, to practise medicine or surgery, dated prior to the passing of the act, granted by any English, Scotch, or Irish university, college, hall, or other person or corporation legally entitled to grant the same, at the time of the passing of the Act.—c. 4.
9. The examination by the councils for the fellowship has no reference to any of the existing corporate bodies.	9. The council's examination to be instituted only on condition that the candidate for the license to practise, shall have previously obtained a degree, diploma, &c. from some corporate body in existence at the passing of the Act.—c. 28.
10. Authorizes practitioners holding " medical qualifications" to sue for charges, &c.—c. 18 and 35.	10. Makes similar provision.—c. 38.
11. Contains no provision for the prosecution of persons engaging in practice without having a " medical qualification ;" but prevents any person who is not in possession of a qualification from holding any public medical appointment. —c. 13.	11. Renders it penal for any one to practise without the license of one of the national councils.—c. 43.
12. All medical apprentices and assistants to be registered.—c. 45.	12. The same.—c. 36.
13. Provides for registration of students, and for their being examined before being matriculated.—c. 46. c. 31.	13. No such provision.
14. Chemists and druggists *may* undergo an examination and obtain a certificate of proficiency ; and the laboratory or shop of such certified chemist or druggist, under certain circumstances, shall be considered as a school of pharmacy.—c. 42.	14. All chemists and druggists *must* possess licenses from one of the national councils.—c. 32 and 43.
15. The appointment of registrars, clerks, &c. to be vested in the Crown ; and the funds arising from the registration to be paid into the Bank of England, and to be under the control of the Lords of the Treasury.—c. 3. c. 2.	15. The appointment of registrars, clerks, &c., and the disposal of the registration fund, to be confided to the councils.—c. 12 and 14.

The preamble of Mr Hawes's draft makes no allusion to the subject of education. The College would recommend the insertion, after " the same shall be" (*vide* the 5th line), of the following words, " duly instructed in the several Departments of Medicine, and,'

The 2d, or interpretation clause, is much simpler than the corresponding one in Mr Warburton's bill, and it appears to be sufficiently precise for all useful purposes.

By clauses 3, 12, and 13 (*Comparative Statement*, No. 15), the appointment and removal of the Registrars are regulated. These functions are committed to the Medical Councils except in the first instance after the passing of the act, when the Registrars are to be nominated by one of the Secretaries of State. The College are of opinion that their nomination and removal ought to be permanently lodged with the responsible Ministers of the Crown, as proposed by Mr Warburton, in order to afford a security to the public for the efficient administration of the act; and that the registrar ought not to be a person engaged in Medical Practice.

The registration of Medical Practitioners, proposed in the 4th, 5th, and 32d clauses (*Comp. Stat.* No. 1), is a most desirable and necessary measure, and is less complex in Mr Hawes's scheme than in the other. Mr Warburton proposes to enrol persons practising the healing art *without* having any qualification, as well as those who *have* it, but in separate lists. This appears to the College unnecessary, and undesirable, as giving a species of encouragement and countenance to unqualified persons, at which presumptuous ignorance and unprincipled selfishness would eagerly grasp. If the qualified men *only* be registered, the public will draw the proper inference as to the others. It is an additional disadvantage of this part of Mr Warburton's arrangements, that it makes necessary a very complicated and expensive apparatus of sub-registrars. As it often happens that diplomas are lost, it will be necessary to provide that the production of certificates, properly attested, of their diplomas having been granted, shall entitle the parties to be registered in such cases.

The 4th clause of Mr Hawes's draft (*Comp. Stat.* No. 8) is evidently designed to place the present race of practitioners, holding diplomas and licenses, exactly on the same footing, as to privilege, with those who shall be licensed under the provisions of the draft (*Cl.* 32), both classes of practitioners having been previously registered in terms of the 4th and 5th clauses. As this is a matter of very great importance, it is desirable that it should be so expressed as to leave no possibility of doubt respecting the purpose of the act; and this may be done by adding to the clause the following words:—"But that each Registrar shall grant such certificates, or such licenses, although the diplomas, certificates, or licenses produced by the applicants may have been granted by Boards whose present powers of licensing do not extend to that part of the kingdom for which the said Registrar is appointed to act."

It appears to the College that the payments for registration, proposed under the 5th clause, ought to be as small as they can be made consistently with the efficiency of the system. There are many medical men in small and ill-paid practice, especially in rural districts, to whom even a small annual payment is of some importance; and when it is considered that the non-payment of the annual sum will infer the loss of qualification, it is obvious that a payment which is needlessly large, would be a very great hardship to many such persons.

The registration of medical students is a matter of importance altogether omitted by Mr Hawes, but introduced very properly into the Bill of Mr Warburton (*Cl.* 31, 46. *Comp. Stat.* No. 13). It appears to the College that a clause similar in principle to the 31st clause of the Bill of Mr Warburton ought to be enacted; that is to say, a clause empowering the Senate and the Medical Councils to frame bye-laws for the matriculation of Students of Medicine, and for the Registration of their several classes. Mr Warburton's 46th clause, prescribing the method of registration, seems unnecessarily and inconveniently minute. The College have for many years conducted such a Register, and feel confident that no plan can be devised which will not be found, on actual trial, to require alterations and modifications to suit it to the circumstances of each school, and to the changes of system which are perpetually occurring. *It will be better,* therefore, not to determine the details by Parliamentary *authority, but to* leave them open to the consideration of the Senate and of the Councils, to whom they may very safely be intrusted. Of the great advantages of a well-digested system of registration, as a check on the irregularities of Students, and on the consequent inefficiency of their professional studies, the College, from their past experience, entertain no doubt. If the system were introduced generally by Parliamentary authority, it would be necessary to provide that the lists of Registered Students should be published annually, with a carefully constructed index; and that no education, which is not so registered, should be accounted a part of the Curriculum.

In regard to the mode of electing a Medical Council for each portion of the United Kingdom (*Cl. 6 to 20. Comp. Stat.* No. 2), and also in regard to the constitution of that Council, there are material differences between the two proposals. Mr Warburton's council of thirty-six appears to be too large for practical efficiency. It seems still more objectionable in containing twelve members not of the profession. The object, of course, is to secure the public against any partial views and interests belonging to the profession; an excellent object, but one which, it is conceived, may be better attained by another arrangement afterwards to be noticed. It appears necessary that a Council, whose office it is to superintend the practical working of the Bill, should consist solely of those who are practically conversant with the state of the profession. Mr Warburton also proposes to devolve on the Council a most invidious and disagreeable task, that of determining which of their number ought to go out of office at each annual period. In all these points the arrangements of Mr Hawes appear to the College to be preferable. His council, containing twenty councillors elected by the profession at large (*Cl.* 6), besides a representative of each of the licensing Boards (of which there are seven in Scotland, *Cl.* 11), would be sufficiently numerous, perhaps *too* numerous, for actual business.

Mr Warburton's plan is decidedly perferable, in proposing a rotation in the Council (*Cl.* 20); while Mr Hawes proposes to elect the Council for three years (*Cl.* 6), and then dissolve it with a view to a complete new election. Perhaps the best way would be to allow the first Council to sit for two years without alteration, and then change one-third of the whole annually. In this case each of the two classes of Councillors ought to have its separate rotation.

The election, according to Mr Hawes, is to take place (*Cl.* 9) " before the Registrar," to which should be added, " or a deputy authorized by him in case of illness or necessary absence." It would also be more precise if, instead of stating (*Cl.* 10) that the " Registrar shall *publicly* ballot," it were enacted that he shall ballot " before two Councillors appointed to superintend the election."

There is an excellent proposal by Mr Hawes (*Cl.* 7), that candidates for the office of Councillor should be regularly nominated before they can be balloted for, and it would be a great improvement on this proposal if the Schedule for nominating them (No. 3) were so expressed, as to make it necessary for those who put them in nomination to ascertain that they are willing to act if elected.

The selection of the Registrars by the Councils (*Cl.* 12) has been already objected to; and the remuneration of the Councillors for their attendance (*Cl.* 14) ought to be subject to some public authority, such as that of the Secretary of State for the Home Department, or the Lords of the Treasury, as proposed by Mr Warburton.

Clause 15 requires the presence of a majority of the whole Council to make their acts valid. This condition, might be found so difficult in actual working, as to render the whole bill abortive. A smaller quorum (six, for instance, as proposed by Mr Warburton), might suffice, and the regular *attendance* of Councillors should be enforced by a penalty. It would also be well that loss of office were incurred by absence from a certain proportion of the meetings during the year (unless from the illness of the party), and that accurate minutes of all the transactions of the Councils were kept (as proposed in Mr Warburton's Bill, *Cl* 21), containing lists of those present at each sederunt, and a full account of all the *res gestæ*.

Clause 17 proposes to place the surplus funds at the disposal of the Councils and Senate conjointly, for purposes connected with the advancement of Medicine, and of Medical Science and Literature. For reasons already assigned, the College are of opinion that the surplus should be applied to reduce the amount of the assessment for the succeeding years.

In concluding their remarks on the subject of the Medical Councils, the College must express their satisfaction with the proposal of Mr Hawes to introduce into them representatives of the existing Boards (*Cl.* 11). The knowledge of the whole subject, which these representatives would bring with them, could not fail greatly to facilitate the transaction of the important business confided to the Councils, while this moderate concession to those licensing medical incorporations, which have hitherto conferred medical qualifications, will be likely to disarm their opposition, and greatly to facilitate the progress of the measure.

Clauses 21 to 29 (*Comp Stat.*, No. 3) relating to the SENATE, are open to many of the remarks already applied to the Councils. By Mr Hawes's draft the members of the Senate are to continue in office five whole years, which is evidently too long a tenure; while the complete annual change of the whole Senate, proposed by Mr Warburton (*Cl.* 24), would be inconsistent with anything like unity of plan in any body so constituted. The number of members proposed by Mr Warburton, thirty-six, appears to the College to be by much too large, while Mr Hawes's number, nine, is perhaps inconveniently small. The fine for non-attendance at each meeting, and the loss of office by not attending a certain proportion of the meetings, are as applicable here as in the case of the councils. The College are of opinion that the introduction into the Senate of a few non-medical members appointed by the Secretary of State, and impartially selected from the three divisions of the kingdom, would accomplish Mr Warburton's object of securing the interests of the public. The functions of the Senate being to secure uniformity of plan in the subordinate boards, and not, as in the case of the Councils, to superintend details, the introduction of non-medical members into the former body seems free from objection. Three non-medical members added to the other nine would make a sufficiently numerous Senate. One of these three, and one member from each of the divisions of the kingdom, might retire annually, beginning after the lapse of the first two years. Of the three members sent to the Senate from each of the Councils, one should be a representative of one of the Licensing Boards.

The power of disallowing the bye-laws made by the Senate (*Cl.* 28. *Comp. Stat.*, No. 4.), proposed to be given to the Privy-Council, would be another important security for these bye-laws being consistent with the public welfare. The College entirely approve of the existence of a check of this description, agreeing, as they do, in the opinion of Mr Warburton, "that in no profession or business is it safe for the State to intrust those who are engaged in that profession or business with the regulation of it, without interposing guards and checks of every kind to prevent the trust from being turned to the advantage of the few, and to the disadvantage of the many."—(*Letter to Dr Maunsell in Dublin Medical Press, 4th November* 1840.)

It appears to the College that clause 23 should be so altered, as to *make it necessary* for the Senate to meet at certain periods in Edinburgh and in Dublin, as well as in London. The expenses of the Senate should be defrayed out of a common fund contributed by the Councils, or in the method provided by Cl. 27, in which the expenses of travelling are, apparently without any good reason, excepted from its provisions. The method proposed in the draft would press with most severity on those Councils most distant from the spot where the Senate assembles.

Clause 25 should provide that the president, or person acting as such, should have both a vote and a casting vote, as in the Councils.

Clause 28 (*Comp. Stat.*, No. 3) confers powers on the Senate to make bye-laws for regulating medical education. It appears to the College, that there are two points relating to the subject of this clause which are so important, that the Senate should not merely be *empowered*, but *enjoined*, to provide for them in their bye-laws. The first of these is the fixing a minimum course of education, without which no candidate should be examined before any Board for a medical qualification. A minimum should also be fixed, to qualify for the license of chemist or druggist. The other point required to be provided for is the qualification of Medical Schools and of Medical Teachers, which it is very necessary should also be distinctly defined, and made universal in its application to all the Boards for examining candidates. The scrutiny of the course of study should be made by some officer acting under the authority of the Councils, who should, after satisfying himself that it is complete, grant a certificate to that effect, addressed to the Examining Board.

Clause 29 should be so expressed, as to authorize and require the Senate to appoint *proper persons* to prepare a Pharmacopœia.

In clause 32 (*Comp. Stat.*, No. 6) extensive alterations would be necessary to render it conformable to the opinions of the College, expressed in the remarks at the beginning of this report. To these remarks the College refer for their judgment as to the Boards for examining, the method of conducting examinations, and the relative advantages of the plans of Mr Warburton and Mr Hawes.

The College cannot but regard it as a great defect in Mr Warburton's arrangements, that in calling into existence a class of practitioners, for whose efficiency they propose to afford a sufficient guarantee to the public, they leave the present system of licensing practitioners, with whatever imperfections there may attach to it, untouched. If there be any of the licensing boards which, for the sake of increasing the number of candidates for their license, require a less complete course of education, or enforce a less rigorous system of examination than would be requisite to insure the safety of the public, there is no provision in Mr Warburton's bill for the abatement of the evil. The licentiates of these bodies will not, indeed, enjoy the universal right of practice conferred on the fellows of the New College, but within certain boundaries their right of practice will continue as at present. Indeed, instead of providing for the remedying of the evils of the present system, a temptation is rather held out by the proposed arrangements to these bodies still farther to reduce their standard of education, and still farther to relax the severity of their examination, as some compensation to candidates for their licenses, in lieu of the more extended rights which the fellowship of the New College is to confer.

As it does not appear that any public benefit can arise from this part of Mr W.'s plan, it may be presumed that it is intended as a boon to the existing boards; but if such be its intent, a boon equally acceptable, and of a much less questionable character as regards the public interest, would be conferred in the adoption of that conjoint system of examination which the College have pointed out and recommended.

Mr Hawes proposes (*Cl.* 32 and 37) *to establish only one grade of qualification* as respects the practice of the profession in all its departments. It will be seen, by consulting the *Comparative Statement* (Nos. 6 and 7), that Mr Warburton proposes *several gradations* of privilege. The College greatly prefer the first of these arrangements. The great object for the public is, to have the general practitioner thoroughly instructed in all departments of the profession. It is on the scientific discrimination and treatment of diseases, in that early stage of them where he alone is consulted, that the preservation or loss of health generally depends. If two grades were constituted, no man of any ambition would be satisfied with the inferior one, which would consequently cease to be duly valued, and might possibly cease to be so carefully conferred as it ought to be. It is quite impossible in practice so to conduct medical and surgical examinations as to make them a just criterion for distinguishing between well-educated general practitioners, and those who are entitled, by their talents and qualifications, to the higher positions in the profession. Such distinctions can be conferred by no authority except that of public opinion, which will necessarily be aided by the degrees and other honorary distinctions conferred upon professional men, as well as by their scientific discoveries, their literary performances, and their general estimation as successful practitioners.

It appears to the College to be a defect of Mr Hawes's Draft that no designation except that of "Medical Practitioner" is given to those who receive the license. A more characteristic designation would be useful merely as a distinctive mark, and it should be such as to convey to the public the idea of merit and of eminence. Some such title as "Fellows of the College of Medicine of the United Kingdom" (see Mr Warburton's Bill, *Cl.* 29) would serve these important ends.

The college of medicine proposed in the bill of Mr Warburton, (*Cl.* 29, 30, and 31—*Comp. Stat.*, No. 5, 6, 7, and 8) appears to be faulty in its constitution. The most respectable practitioners now existing in this country could not obtain admission to it, except by ballot. In all prior legislative changes, it has been usual to pay the greatest possible regard to the interests of such parties. They ought not merely to be protected in the enjoyment of their present privileges, but freely admitted, and that without the ordeal of the ballot or of fresh examinations, to that equality of professional advantages which it is the object of all reformers to secure to the whole medical profession.

Clause 34, which is intended to exempt dentists and cuppers now in practice from the penalties of the proposed act, will be altogether unnecessary, if unqualified practitioners of every description be permitted to practise without being liable to prosecution, as recommended in the prefatory remarks.

Clause 36 (*Comp. Stat.* No. 12,) relating to the medical assistants of practitioners and of chemists and druggists, will require emendation. Subordinate assistants, who are not apprentices, are often employed in drug-shops and in the private laboratories of practitioners, for the purpose of acquiring a knowledge of the qualities of drugs, and of the arts of pharmacy and prescription. These are undoubtedly "assistants," and they are so circumstanced that they cannot possibly hold a qualification under the act. So long as they hold a decidedly subordinate position, and act under the directions, and on the responsibility of the heads of the establishment, no injury can arise from this practice, any more than from employing apprentices. The case of partners and salaried superintendents acting on their own responsibility, is altogether different, and to them the clause may fairly be made applicable.

Clause 37 would be greatly improved by the addition of some such sentence as the following, which is nearly verbatim the 13th clause of the Bill of Mr Warburton:—" And no persons who do not possess certificates to practise the Art of Medicine shall be deemed capable of acting in any part of the United Kingdom in the capacity of a Physician, Surgeon, Apothecary, Surgeon-Apothecary, Accoucheur, or other Medical Officer, to any Hospital, infirmary, dispensary, lunatic or other asylum, lying-in-hospital, gaol, penitentiary, house of correction, house of industry, parochial or union work-house, or poor house, parish, union, or other public establishment, body, or institution, or to any society for affording mutual relief in sickness, infirmity, or old age, nor in the capacity of a Physician, Surgeon, Assistant-Surgeon, or Apothecary, to the British Army or Navy, or in the service of the East India Company." If the penalty for practising without a license be abrogated, in conformity with the suggestions of the College, it will be very necessary that this clause should be adopted.

The right to recover professional charges in courts of law, which it is the object of clause 38 to confer on the qualified practitioner (*Comp. Stat.*, No. 10), should not be given to the unqualified. For this reason the College propose to introduce after the word " Medicine" (line 4th) the words " but for no others."

Clause 43 (*Comp. Stat.*, Nos 11 and 14), so far as it relates to practitioners, is inconsistant with what has been advanced in the prefatory part of this report. The College, therefore, would wish the whole of it to be omitted, except what relates to Chemist and Druggist. But they would propose as an amendment of the 41st clause, to introduce, immediately after the word " aforesaid" (line 11th,) the following words: " Or shall fraudulently assume any title belonging to those who are licensed under the act, without having acquired a legal right to the same."

There are some small matters, of little more than verbal importance, which are subjoined, as their introduction in the proper place would have interrupted the course of the remarks.

In clause 11 the designations of the Scottish licensing bodies will require to be differently worded. In some of the Universities there is no body known by the name of the Faculty of Medicine; and the Colleges of Physicians and Surgeons, being possessed of popular constitutions, conferring on all their members equal rights, will claim the privilege of appointing their own representatives instead of delegating that power to any Council. The same remark is applicable to the constitution of the College of Surgeons in Ireland. The clause as it respects Scotland may be amended thus: " It shall be lawful for the Senatus Academicus in the Universities of Edinburgh, Glasgow, St Andrews, and Aberdeen, and for the Royal College of Physicians and the Royal College of Surgeons of Edinburgh, and for the Faculty of Physicians and Surgeons of Glasgow." &c. &c.

In clause 16, the transposition of a sentence seems necessary to bring out the sense intended to be expressed. The words " at which a majority of the existing members of the Council shall be present" (see lines 6 and 7), should be brought in after the word " meeting" in line 8th.

In clause 32, page 14, line 11, the word " either" should be exchanged for " any one."

In clause 45 occurs the expression " Petty Sessions," a term not employed in Scotland. The clause may be amended by the introduction, after the words " Victoria the First," of the following: " Or one of the Justice of Peace Courts in Scotland."

The schedule No. 3 may be thus amended, to suit it to the remarks already made on Cl. 7 of the Draft of Mr Hawes: " Having ascertained that the following persons, if elected, are willing to act as members of the Medical Council for [England], I hereby nominate them as fit and proper persons to be returned in that capacity at the ensuing election."

A schedule for the registry of Students should be added to those at the end of the draft.

On the whole, the College rejoice in the acknowledgment, that there is much matter of a beneficial description in both of the plans of medical legislation which have been under their consideration. In the comments upon them which they have presumed to offer, the College have kept steadily in view the important principle, that the profession at large is entitled, on the most undeniable grounds of public utility, to a measure of improvement calculated to confer equal privileges on all medical men who are liberally educated, and to enforce a high standard of education upon all, as the condition of obtaining such privileges. In admitting that existing corporate rights can be respected only in so far as they are compatible with such a scheme, they have contended, that, in whatever degree they *are* compatible, they should be amalgamated with the proposed new arrangements, and all that is good in them held sacred. If other medical incorporations and associations will offer their suggestions in a similar spirit, the College have no doubt that the best parts of both schemes may be blended together in such a way as to secure the enactment of a sound and useful measure of medical reform, satisfactory to the profession, and highly beneficial to the public.

The late Mr Musgrave, Student of Medicine.—It is to us always a grateful although a melancholy duty to do justice to departed worth and talent, whether in a veteran, or, as on the present occasion, in a juvenile member of our profession.

Died at 9, Regent Terrace, Edinburgh, in the 20th year of his age, of typhous fever, contracted in the discharge of his duty as physician's assistant at the Royal Infirmary, Mr William Musgrave, eldest son of Dr Musgrave of Antigua. Mr Musgrave received his classical education at the Grammar School of Hackney, where he acquired the approbation and esteem of his teachers and school-fellows by his assiduity and amiable manners. He distinguished himself as one of the most diligent and successful students at King's College, London, and came to Edinburgh at the commencement of the present session to finish his medical education, and to take his degree at the University. His melancholy and premature decease has occasioned an extraordinary sensation amongst our medical students, which has been extensively felt by the many distinguished members of the medical profession. The remains of this amiable and talented youth were interred on Saturday the 28th November, in the burial ground at Newington, and were accompanied thither by the Principal and medical Professors of the University, by several of the Fellows of the Royal Colleges of Physicians and Surgeons, and by the most intimate of his fellow-students, who will long cherish a remembrance of the moral worth and intellectual attainments of their lamented associate.

Erratum in Dr Smith's paper on the Diseases of Peru in No. 145.
Page 359, line 22, *for* arms *read* anus.

EDINBURGH:
PRINTED BY JOHN STARK,
OLD ASSEMBLY CLOSE.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. APRIL 1841.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Jaundice from Non-Elimination, together with Remarks on the Pathological Condition and Chemical Nature of the Bile.* By W. H. LOWE, M. D., M. R. C. S., Lond., late President of the Royal Medical Society of Edinburgh.

DURING the time I was resident in the Royal Infirmary of Edinburgh, my attention was chiefly directed to a subject, which of late years has attracted some attention as an object of pathological inquiry. The disease to which I allude is Jaundice from Non-Elimination. But in entering on the consideration of this subject, it will be necessary briefly to advert to some other forms of jaundice, arising from causes more demonstrable, and better ascertained.

The sudden appearance of jaundice, arising without premonitory symptoms, or supervening on some other and long-continued disease, was usually referred to a doctrine prevalent some years ago, and even supported by some in the present day, viz. spasm, or the very opposite condition, paralysis of the gall-ducts. This theory is so purely hypothetical, and so little supported by direct evidence, that I am constrained to regard it as a mere loop-hole, of which the older physiologists were accustomed to avail themselves, and to which they were driven by the erroneous idea that the liver was the sole agent employed in the formation of bile. (Bostock, Phys. p. 370). The observations of physiologists go far to show the muscularity of structure in the middle coat of the bile-ducts; but, granting that their reasoning were founded on firmer grounds than those of analogy, the supporters of the doctrine of spasm have yet to prove that the muscular contractility is actually capable of being excited into action by merely mental impressions. I do not deny, nevertheless, that the presence of gall-stones may by direct contact produce irritation, and consequent contraction of these tubes, as Rudolphi, Monro, Tiedemann, and Müller, have shown that, by such direct application of stimuli, contraction may be distinctly but slowly produced. This is, however, denied by Saunders. At the same time, the existence even from this cause of these spasms (les coliques hepatiques) appears to rest more upon hypothesis than actual observation; and it is remarked by Bouillaud, that in those instances in which biliary concretions have been found after death, the patients had never previously complained of any uneasiness, or given any indication of their presence, (Arch. Gen. de Med.); and this is fully corroborated by Andral (Clin. Med. Tome iv.) The opinions of Abercrombie on this point are probably familiar to all; and I only allude to them as agreeing with those already expressed.

The next cause of jaundice, which I shall notice with equal brevity, is where there exists a "mechanical obstruction to the flow of bile, already secreted," as in the case of gall-stones in the ducts; a loaded state of the colon (Dr Abercrombie); inflammation of the mucous membrane of the duodenum, extending to the mouth of the common duct, and causing obliteration, (Dr Marsh); the occasional existence of abdominal tumours, enlargement of the pancreas, &c.; and I may here mention the well-known case of Dr Saunders, in which some gooseberry-seeds proved the cause of obstruction. The ducts being thus obstructed from any cause similar to these enumerated, an accumulation of bile takes place, and a gradual regurgitation within their canals, while the absorbents of the liver, stimulated into increased action by the presence of the fluid, the lately secreted substance is again taken into the circulation, producing the deep-yellow colour so peculiarly characteristic of the disease. (Ferrus, Dict. de Med.) That absorption does take place in this way, and under such circumstances, is, I think, shown by the experiments of Dr Saunders, and also by one of my own, to which I shall presently advert.

At first sight the difference between these causes referable to mechanical obstruction, and those in which the process of elimination appears to be merely suspended, seems one of little importance; and the result being in either case the impregnation of the blood with bile, such a distinction may seem more curious than *useful*. But further examination will show, that fatal consequences are far more common in cases where jaundice proceeds from non-elimination, than in those where the biliary matters have been secreted, and subsequently reabsorbed. The only explanation of this physiological fact is that offered by Professor Alison, (Ed. Med. and Surg. Journal, Vol. xliv.) who advocates the opinion, that, in the process of reabsorption, the biliary matter undergoes some change, which renders it less injurious to the animal economy; and quotes in support of this idea the assertion of Dr Prout, that nothing is absorbed into the living body without having previously undergone a process analogous to that of digestion, which idea is strongly supported by the great similarity of the contents of the lymphatics at different times and in different parts of the body, as ascertained by Magendie. The greater facility also with which some substances, and the difficulty with which others are absorbed into the body, appears to me to offer an additional explanation; because, while the inert colouring matters are readily taken into the system, the poisonous resin or cholesterine may be absorbed only in small proportion. Either of these views will, I think, suffice to explain the more frequent occurrence of coma and death in cases of jaundice from non-elimination, than in cases where we have reason to believe that the biliary matter has been eliminated, but again absorbed.

With these remarks on jaundice in general, I shall now turn my attention to that particular form of it which has been termed jaundice from non-elimination. This suspension of the biliary secretion, it will be necessary to state, may be dependent either upon disease of the liver itself, whereby that organ is rendered unfit for the performance of its ordinary functions, or it may take place, as there is every reason to believe, spontaneously, and without any appreciable lesion of that organ. Hence a very obvious division of the subject. To the first class belong those cases in which there exist inflammation and congestion of the liver, abscesses of the liver, scirrhous or malignant diseases in general, or fatty degenerations in the gland. All these diseases may, however, take place to a very considerable extent, without altering the appearance and physical conditions of the bile; and this secretion may, on the other hand, be obviously vitiated without any marks of disease in the liver. (Andral).

With regard to inflammation of the liver, it has been remarked by Dr Abercrombie, that jaundice is most frequent when the inflammation is situated on the concave surface of the liver. Annesley, Cheyne, and many others, have remarked the frequent occurrence of jaundice in fever, and indeed every one conversant with practice must be aware that its occurrence under such circumstances is by no means uncommon. Local congestion of internal organs in cases of fever is, as every one is aware, extremely common; hence, congestion of the liver, at least in some instances, might naturally be looked for, and this in yellow fever is actually the case,—the liver being usually found to be distended with adventitious matters. The same is doubtless frequently the case in the fever of this country, though, perhaps, in a less severe degree. The yellow gum, as it is popularly termed in Scotland, or the jaundice of new-born children, has been referred by some, (Dr Powell, Dr Burn, &c.) to a too viscid state of the bile; an opinion I have not thought it necessary to discuss. Breschet has with more reason attributed it to the congested state of the liver; and the observations of Bouillaud, Andral, and Billard, show that this organ in new born infants is frequently, if not generally engorged and distended. I may remark, that it is not always easy to distinguish between the congestion of some authors and the inflammation of others,—those of congestion of Andral bearing much resemblance to those of inflammation of Abercrombie.

In illustration of those cases where the suspension of the biliary secretion takes place, without any appreciable disease of the liver, I shall briefly insert the following

Case.—Jane Stephenson, aged 30, admitted into the Royal Infirmary, October 1839. This patient was admitted for the relief of some pectoral disease under which she laboured, and had formerly been treated in the same hospital for rheumatism. A short time after her admission she was seized with jaundice, and after a few days died in convulsions, produced, there can be little doubt, by the poisonous influence of biliary matter upon the brain.

On examining the body, all the tissues were found of a deep yellow colour, and this especially observed in the cartilages of the articulations. The heart and membranes were a good deal diseased. The liver was small, especially the left lobe; it weighed only one pound eight ounces; it was firm, and without any apparent change in its textures, and presented a yellowish tinge. A probe introduced into the *ductus communis choledochus*, and passed downwards, entered the duodenum readily. On tracing the bile-ducts into the surface of the liver, they were found pervious throughout and empty of bile. The kidneys were healthy.

For cases of a similar nature I may refer to those related by Abercrombie, Marsh, Alison, (Med. and Surg. Journ. Vol. liv.,) and, in particular, to a paper by Dr Griffin in the Med. Gazette for 1833—34. There is also in the same volume another case related by Dr Aldis. In the cases mentioned by Dr Griffin, this fatal form of disease appears to have prevailed amongst several members of the same family; one only recovering from it. In all these cases, as well as in many occurring under the observation *of Mr Twining* in India, " no cause could be assigned for the disease." The opinion, that jaundice, in some cases at least, is dependent simply upon a suspension of the ordinary function of the liver, appears to have obtained, in the time of Boerhaave and Morgagni; and it is somewhat amusing to observe, that Dr Saunders only alludes to it to remark, that such a theory is so little tenable as to render it unnecessary to refute it. It is upon suspension of the biliary secretion that I would explain the case I have related, as well as those mentioned by the authors above-named, as in these cases no disease of the liver could be detected on examination, while the retention of the biliary matter in the circulation was evidenced by the yellowness exhibited, and by the coma and convulsions which preceded death. To this cause I would refer those frequently cited, of patients who have suddenly become jaundiced after some sudden shock or mental emotion. I might quote the well-known instance of this kind, in which a young man died at La Pitié, deeply jaundiced, in consequence of a musket having been pointed at his breast. Even where gall-stones have been found after death in cases of this kind, it appears more than probable that, at least in some instances, the jaundice was coincident, and not consequent, upon the existence of these concretions.

To admit the existence of this suspension of secretion is to recognize an important physiological principle, viz. that the blood is the general material from which every secretion, however complicated in its nature, is derived, and that the office of the glandular system is not to secrete, but merely to eliminate from the general mass, at least the elements of the various secreted fluids. The great variety of substances which have been at different times and by different chemists detected in the blood, tends to give the highest probability to this theory. Cerebrine, for example, the peculiar and principal constituent of the brain; albumen, fibrine, the base of muscle, salts, biliary and fatty matters, have been shown by Prevost, Dumas, Chevreul, and others, to exist in the blood; and in an experiment of my own, performed upon a dog, where the *vena portæ* was tied, scales of cholesterine, visible to the naked eye, were found in the blood of the heart, and especially in the fluid of the abdomen. The actual observations of authors upon the condition of the blood in jaundiced persons will here be of importance in guiding our speculations; and I gladly avail myself of those collected by M. Le Canu, (Etudes Chemiques sur le Sang Humain). "Parmi les chimistes," says he, "les uns pretendent que le sang des ictériques renferme toujours de la bile, d'autres, au contraire, qu'il n'en contient pas, et doit sa couleur à la presence d'une matière colorante particulière; d'autres enfin adoptant une opinion, en quelque sorte mitoyenne, pretendent que sans contenir de bile, le sang des ictériques contient ses principes colorants. Parmi les premiers, on compte MM. Orfila, Clarion. Parmi les seconds, on compte MM. Thenard, Lassaigne. Parmi les derniers, on compte MM. Chevreul, Collard de Martigny, F. Boudet, Le Canu. To the latter I may also add MM. Solon, (Bull. de Therapeutique.)

Admitting, however, that these various substances have been found in the blood, the process by which they are converted into the different secretions, as bile, saliva, urine, &c. eludes at present the most careful scrutiny. It appears to me probable, that while we are right in refusing to the glandular system the power of converting the blood into the several products of that system, the elements of those substances may, in passing through the glands, be combined in consequence of some peculiar influence there exerted, and the result be the substances known to us, as bile, saliva, or any other. It was in connection with this subject that I made the following experiment.

I took a mongrel dog of moderate size, and, with the assistance of Mr Spens, placed a ligature upon the *vena portæ.* A vessel, which, at the time, was believed to be the hepatic artery, was also secured. The protruded viscera were then returned, and the wound brought together by means of sutures. The dog on being released leaped from the table and shook himself. Three hours after I visited him, when he recognized me, answered to my call, and showed a desire to follow me. Eight hours after the operation I again saw him. He answered to my call, and was able with some difficulty to follow me to my residence, at a short distance from the place where the operation was performed. At this time the conjunctivæ were slightly injected, but not at all yellow. Twenty-two hours after the operation he was very stupid and sleepy, gave no indications of pain, and did not respond by any motion when called; he had taken no food, and had not passed either fæces or urine. He was found dead in an easy posture thirty hours after the operation. On examination, the conjunctivæ were perfectly white, as were also the cartilages of the ribs and integuments of the chest. The liver was deeply tinged with bile, as was also the whole of the abdomen. The gall-bladder was distended. The ligature upon the *vena portæ* was rather loose, but appeared to have answered the purpose of arresting the flow of blood, as there was a firm coagulum within, and a little purulent matter. Instead of the hepatic artery being tied, it was found to be the *ductus communis choledochus*, and hence the distension of the gall-bladder. The blood of the inferior *venæ cavæ* and other abdominal vessels contained floating particles, easily seen by the naked eye, which I was able to pronounce to be cholesterine. The vessels being tied above and below, the heart and lungs were then removed, and washed carefully in cold water. On opening the heart, the blood contained had an oily appearance, and minute scales were visible on the surface. As the blood of the heart had lost nearly all its serum, I added distilled water at a temperature of about 96°; after some time, small scales, scarcely visible to the naked eye, were observed to float upon the surface, with a larger one in the centre, which could be distinctly recognized as cholesterine, and, on placing a drop containing these scales under the field of an excellent compound microscope, no doubt remained as to their nature, and I did not deem any further examination of them necessary. The blood and fluid of the abdomen contained, as I have already said, a still larger proportion of this substance. The bile, which appeared healthy in its characters, contained, when chemically observed, the ordinary quantity of cholesterine.

This experiment, though on the whole satisfactory and confirmatory of the views I have maintained, is nevertheless open to numerous objections. The common duct was secured instead of the hepatic artery; hence M. Denis, who denies the formation of cholesterine in the blood, might contend for its having been absorbed. It is, however, nearly evident that the absorption of the biliary matter did not take place to any great extent. The examination was not made until some hours after death, and should have been much earlier performed. The experiment would also have been much more satisfactory had I abstracted blood from the jugular vein during life, and after the symptoms of stupor and coma had become evident.

M. Simon de Metz found it impossible to perform the foregoing experiment on the dog, which is certainly one of great difficulty. I repeated it on a subsequent occasion, but the animal died too soon after the operation to draw any conclusions from it. M. S. de Metz, however, seems to have arrived at some interesting and well supported facts in the course of his experiments performed chiefly on pigeons.—(Annales des Sciences, Tom. xiii.) In these animals which do not possess a gall-bladder, there are two hepatic ducts, one of which pours out bile almost continually; the other, which opens into the great intestine, is generally found empty. If these ducts be tied, the liver becomes engorged with green biliary matter, which soon pervades the neighbouring tissues, the intensity of colour depending on the length of time the animal survives. There is one remarkable fact observed in this case, viz. that, about twenty hours after securing the hepatic ducts, the cloaca was observed in all instances to be filled with green matter similar to that seen in the distended liver, while the intestines contained none. M. S. de Metz considers that this green biliary matter was secreted by the urinary organs, which took on an increased action, at the same time that the natural secretion of these organs had undergone an alteration in its nature; thus strikingly corroborating the observations of Prevost and Dumas, that, in cases of diseased kidney in the human subject, the secretion of the liver is not only increased in quantity, but altered in its nature. In five cases of granular disease of the kidney which I examined in connection with this subject, the contents of the gall-bladder were in them all plentiful in quantity, and certainly altered in their nature, especially with regard to the proportion of red colouring matter which they contained.

It naturally becomes a question as to what is the poisonous principle contained in the bile; and the experiment of Magendie, who injected seven grains of bile into the veins of a dog, proves nothing on this point. It does not appear probable that the colouring matter is in itself the injurious principle, as we frequently see persons deeply jaundiced who yet suffer little or no inconvenience from it. I have examined the contents of the gall-bladder, in some instances tolerably well filled, and found that the secretion consisted almost entirely of colouring matter and mucus, but no cholesterine, and very little resin. Absorption taking place, therefore, under such circumstances, would not, I conceive, be immediately injurious, as while the proportion of colouring matter was increased, so as to tinge the whole body, it would yet furnish no criterion of the presence of the more poisonous biliary matters. With regard to the cholesterine, its existence in the blood has been asserted and denied by equally able chemists. I have myself examined the blood of two persons with a view of ascertaining its existence, without being able to detect it, and in the bile of five healthy individuals, (two of them being men killed while in perfect health), cholesterine was wanting in two. I am therefore inclined to believe that not only is this substance occasionally wanting, but that its elimination is usually synchronous with its production. If it were found in the blood in such proportion as it appears to have been in the experiment on the dog which I have related, it would, I think, be rapidly fatal, merely from the mechanical obstruction which would be caused to the circulation by this scaly and peculiar substance. In the bile of the pig, which in many respects resembles closely that of man, I have never observed it; in that of the ox and sheep, it is in very small quantity, while in that of the dog and man it is in general very abundant.

I have now endeavoured to take a hasty and general view of a somewhat extensive subject; but, as I wish to add some remarks on the pathological conditions of the bile and its chemical constitution, I must leave this part of my subject thus imperfectly discussed.

Such observations as we possess on the morbid and healthy conditions of the bile, as found in the human body, are certainly extremely meagre, and, from the fewness of their number, and *their frequent* want of relation to any ascertained and previous condition of the patient, can do little towards establishing any mutual connection between disease and the state of this secretion. Besides observations upon the bile taken from the human body in more than thirty cases, I have repeatedly examined the same fluid in the ox, dog, pig, sheep, and rabbit, both with a view to their physical properties, and to their chemical nature ;—and first, with regard to its physical characters. The colour of bile taken singly, can hardly be said to furnish any just criterion of its healthy condition, yet, in conjunction with other circumstances, it furnishes one of the best evidences of the vitiated or normal state of this secretion. Dr Baillie thought that the differences in colour which this fluid presents ought not to be regarded as indications of disease, because they are so commonly met with ; but when we reflect how rarely we have the opportunity of examining the state of this secretion in healthy individuals, this argument can have little weight. Healthy bile is described (I believe, most improperly,) by many as a green fluid,—but it has already undergone some change when it presents this colour. Mr Marshall mixed thick black bile with vinegar, and formed a green mass, like chopped spinach, (Dr William Thomson). Dr Powell also mixed yellow bile with gastric juice, and it became green ; and I have myself observed that bright yellow bile, recently vomited, rapidly became green by exposure to the air, probably from the same cause. But that some variety of colour may exist without morbid alteration seems probable, from observing the bile of two oxen, killed at the same time, in one of which it was green, in the other of a reddish colour. In four cases, in which there was no reason to suspect disease, the bile of man was, " en masse," of a nearly black colour, staining white paper of a " bright orange-yellow." (Werner's Nomenclature). The bile of the ox is of a green colour, but occasionally of a reddish tinge ; that of the dog exactly resembles that of man in appearance. The bile of the pig is of a light reddish colour, resembling sherry wine when diluted ; that of the sheep is dull olive-green ; of the rabbit, light reddish-brown. These characters are constant, and easily distinguish the one from the other. The effect produced in the colour of all these by the addition of an acid is also remarkably different. On adding muriatic or other acids to the bile of the pig, for example, an instantaneous precipitate is formed, of a buff-yellow colour, while the same substance, added to that of the sheep, very slowly produces a dark dull-green precipitate. The same difference in the rapidity of chemical action is observed, when the analysis is carried on by means of acetate of lead,—thus forming a very decided distinction between this secretion in different animals.

The consistence of this fluid does not differ, I believe, remarkably amongst the lower animals, if we except that tenacity which depends upon the quantity of mucus contained,—the latter substance often being very abundant, at other times bearing only a small proportion to the rest of the biliary matters. The structure of the gall-bladder is different in different animals; that of the sheep, for example, readily permitting the transudation of the bile, while that of the ox completely retains it. Albinus believed that the gall-bladder secreted a bile of its own, chiefly from contemplating its honey-comb structure; but this opinion is not, I believe, entertained in the present day. As observed in the human subject, under the usual course of *post mortem* investigation, the bile presents almost every degree of consistence, being at times perfectly limpid, at others, so tenacious as to admit of being drawn out into threads, in which state it has been described as resembling frog-spawn. On one occasion, I observed it so nearly solid as to take, in some measure, the shape of the gall-bladder containing it.

The average quantity found in the gall-bladder has been stated by Dr Saunders to be one ounce. I believe half that quantity will be nearer the truth. The greatest quantity I have ever seen is two ounces. If, however, we admit into our calculations those extraordinary instances where the quantity has exceeded two Scottish pints, and, according to some continental authors, even a far greater quantity, the average might be placed somewhat higher; but as these form very rare exceptions, they are, I think, better left out of consideration.

The taste of bile is intensely bitter, followed by a slight degree of sweetness, which depends, according to M. Demarcay, on the manner in which it is applied to the palate and tongue, and the greater or less degree of sensibility in these parts, and also on the consistence and size of the morsel tasted. That form of bile named picromel, owes, I believe, its sweetness to the portion of acetate of lead which the sulphuretted hydrogen is insufficient to remove.

The prevailing idea, that bile is a substance which readily undergoes putrescence, has been shown by Dr Saunders to be incorrect, and such has been my own observation. On the other hand, in two persons who had died of fever, and in both of whom the quantity was large, the contents of the gall-bladder were decidedly putrid twenty-four hours after death. The weather was, however, at the time, unusually hot. Dr Davy's remarks regarding the non-existence of free carbonic acid in this secretion, are, I think, perfectly satisfactory, and I merely allude to them. (Researches Phys. et Anat. Vol. ii.)

The presence of a sediment or of grains is extremely common, and is not, I believe, an unhealthy indication, except where these bear a very large proportion to the rest of the fluid. In one instance, these grains appeared to have a nucleus, and, I might almost say, were incipient calculi. It seems probable that this may be the origin of those concretions which are formed of successive layers of biliary matter. In the rabbit, the coats of the gall-bladder are perfectly translucent, and easily admit of these particles being seen floating in a clear yellow liquor.

Considering that my own opportunities of observation were somewhat limited, I collected more than 140 cases in which the biliary secretion had been observed by Bright, Annesley, Thenard, Andral, Orfila, Chevalier, Hodgkin, Graves, and Stokes, and many others, in which the disease under which the patient had laboured, the age, sex, and condition were ascertained, as well as the physical characters of the bile, and where I could meet with it, the chemical composition. But the remarks of all these, except a very few by Orfila, Chevalier, and Thenard, were too general and vague in their nature, to afford more than occasional corroboration of my own remarks. The necessity of possessing some practical form of general analysis, before we can attempt any classification of morbid appearances, must therefore be obvious, and this, I hope, in the ensuing observations on the chemical nature of the bile, to be able to afford.

Thenard has remarked, that in a case of diseased liver, the quantity of biliary matter was small, but little can be deduced from a single observation. I have, however, remarked, that where the patient has been long wasted by disease, especially by *phthisis pulmonalis*, the contents of the gall-bladder have been little else than colouring matter and mucus. In several cases where the vitiated condition of the bile has been very evident, the red colouring matter described by Muratori and Collard de Martigny has been so abundant as to give a brick-red colour to the bile. It somewhat resembles blood, but boiling and the addition of alcohol do not alter it. It is most easily obtained from the bile of the pig by precipitating the biliary matter by means of sugar of lead, and then passing sulphuretted hydrogen through the fluid. The red substance is then easily distinguished by its conspicuous colour, but is in very small quantity.

The relation which, according to Muratori, exists between the quantity of soda and that of cholesterine, has hardly been verified by myself. The cholesterine is most abundant in human bile, while the soda is, I believe, far more abundant in that of the ox, in which, as in all the lower animals except the dog, the cholesterine is in very small quantity. I have observed in some instances, that human bile, on being exposed to the air, has become covered with scales of cholesterine, though the quantity of soda must have remained the same as at the first. Hence it would appear that the use of the soda is not merely to hold in solution the cholesterine. With regard to the latter substance, out of twenty-five cases examined by myself, five only contained cholesterine, and of these five, two were undoubtedly healthy. With these remarks, I shall now pass on to the consideration of the chemical nature of the bile.

In considering the chemical nature of the biliary secretion, it is not my intention to offer an exact analysis of this substance, but rather to state the results of my own experiments, which have been conducted on a somewhat extensive scale. Before detailing any individual experiment, however, it will be necessary to glance at the commonly received opinions regarding this substance. The saponaceous properties of bile were generally admitted; such at least was the opinion of Boyle, Boerhaave, Fourcroy, Van Boehan, Ramsay, Verheyen, Cadet, and others, until a modification of this opinion was introduced by Thenard. Its physical properties seem alone sufficient to establish this opinion. Its homogeneous nature, its viscidity, its great solubility in water, its attraction for moisture, as well as its well-known property of removing grease spots, and still more the chemical experiments of M. Demarcay, and those made by myself, all tend to prove that it is really a soap having soda for its base.

Before entering on examination of the views of Thenard, I shall allude to a substance contained in the bile, and mentioned by Fourcroy as a substance resembling albumen, considered by Dr Baillie as analogous to white of egg, and by Dr Bostock as a substance intermediate between albumen and mucus. Dr Powell also believed that the bile not only held this substance in solution, but exerted such an influence upon it, as to render it unsusceptible of the action of ordinary reagents. I am not aware that the latter opinion has ever received corroboration; but I have on more than one occasion observed that alcohol was insufficient to precipitate or coagulate this substance, even when used in considerable quantity; yet when this muco-albuminous matter has been precipitated by muriatic acid, and again dissolved in alkalies, it did not exhibit any peculiarities that would entitle it to be considered as a peculiar principle, and in all respects answered to the character of mucus. The saponaceous properties of bile were, as I have said, generally admitted, until Thenard endeavoured to show that this opinion was incorrect, and that bile contained a peculiar substance, (picromel), to which its solubility and tendency to froth when agitated was owing. This opinion was immediately adopted, and the same view is stated by Dr Ure in the following words, " The property which caused bile to be considered as a soap is owing to the soda, and to the triple compound of soda, resin, and picromel." Dr Fyfe also, in speaking of picromel, says *that it* has the power of dissolving resin; indeed, it is chiefly by *its agency that* the resin is held in solution in bile, for when united with it, it forms a substance, having the peculiar flavour of bile. The picromel, however, is not in sufficient quantity to keep the whole dissolved, so that the free soda must also exert its influence. —And again, " Picromel vel in aqua vel alcoholi solubile est, neque crystallos format, neque fermentationem subit, et simul cum soda et resina bilis fluidam constituit quæ neque acidis nec alkalinis, nec terris iterum separari potest. Picromel igiter sodæ liberæ et albumini junctum, resinam bilis fluidam tenere videtur."

I shall endeavour to show, by a few simple but conclusive experiments, that not only is this picromel improperly regarded as a peculiar principle, but that the resin of Gmelin, the picromel of Thenard, and the " choloidique et choléique acides" of Demarcay are in fact one and the same thing.

The analyses furnished by the different chemists on bile are extremely complicated, and the substances found by them very numerous; that of Gmelin containing twenty-seven different matters as entering into the composition of bile. This, however, includes the oleic and margaric acids, and cholesterine, which are not noticed by Thenard. M. Demarcay, in his paper on the bile, (Annales de Chimie et de Phys. Tom. lxvii.) has presented us with views upon this subject, which are, I believe, altogether original; and it is from the perusal of his paper that I was led to make the experiments that follow; but while his analysis is one of considerable value, it appears to me open to certain objections, which I shall point out.

His analysis is as follows;—1. a substance soluble in water azotized; 2. a substance insoluble in water not azotized; 3. taurine; 4. chloride of sodium. This general division (for it does not appear to be the intention of the author to offer an accurate analysis) is, I believe, incorrectly stated as regards the bile of man in the following points. In the process of boiling the bile with acids, there is a peculiar volatile principle evolved during the first part of the ebullition. This is mentioned by Gmelin as a substance having the odour of musk, and by Berzelius and others as " a volatile odoriferous principle." It is at the same time remarked, that " son existence n'est conclue, au reste, que l'odeur exhalé par l'eau qui passe à la distillation." This odoriferous principle is, however, so constantly evolved from the bile of all animals which I have examined, and its smell is so penetrating and peculiar, that it cannot with propriety be omitted. We must therefore add to the substances enumerated by Demarcay " a volatile odoriferous principle."

The two last substances mentioned are taurine and chloride of sodium. I have examined in a considerable number of cases the bile of the human body, but in no instance have I obtained a trace of taurine; while, therefore, it is properly named as a constituent of the bile of the ox, cat, and those animals in which it has been found, it cannot be reckoned among the components of human bile. The last substance mentioned is chloride of sodium. I am at a loss to know why this salt should be noticed, and the other inorganic saline matters omitted, more especially as the greater proportion of this salt must, from the nature of M. Demarcay's experiments, be regarded as a product rather than an educt. Thenard regards chloride of sodium as forming a constituent of bile in its normal condition. The phosphates of lime, potassa, and soda, and many others, have been shown by Gmelin and others to be present, and the former of these, in particular, I have had occasion to notice frequently. The results of the analysis of M. Demarcay may be therefore more correctly stated, as 1. a volatile odoriferous principle ; 2. a substance soluble in water azotized ; 3. a substance insoluble in water not azotized ; 4. inorganic salts ; 5. taurine. But I shall have occasion to differ still more widely from M. Demarcay in the subsequent remarks.

I shall detail the following experiment, which is nearly that of M. Demarcay, the only difference being, that he made use of the dried alcoholic extract, and I of the fresh bile.

Experiment 1.—I took a quantity of ox bile, and after precipitating the mucus by means of alcohol, I added a portion of the hydrochloric acid of commerce. A precipitate of a yellow colour was produced, but a complete separation of the biliary matter did not take place till heat was applied. I placed the whole in a flask, and boiled it for two hours. As soon as ebullition commenced, a peculiar and offensive odour was given off, but after some time ceased. A green oily matter was seen collecting on the surface of the fluid, which gradually sunk to the bottom of the vessel, causing such violent succussions as to render great caution necessary in boiling. This green matter gradually assumed a darker colour and greater consistence resembling pitch, while the fluid became clear and had a purplish tinge. The concrete mass was then washed in distilled water and dried ; it was then of a dark-olive brown colour, and easily pulverized. A portion of this was dissolved in alcohol, and agitated with ether, in order to remove any portion of oleic or margaric acids. It was then filtered and dried on a water-bath, when a mass of shining appearance and brittle resinoid fracture was the result, which, when pulverized, was of a yellow colour. The clear fluid from which the pitchy mass had been separated was then placed on the sand-bath, when, after a short time, globules of an oily nature and of a reddish-brown colour were observed to deposit themselves on the sides of the vessel and on the apices of the crystals of chloride of sodium, which were now abundantly formed. The whole then presented a thick mass of a dirty colour, in which nothing could very distinctly be made out. I next added strong alcohol in large quantity, when after some time a deposit of beautiful crystals were observed, which proved to be sulphate of lime and taurine, or "asparagine biliaire."

Repeating this process on the bile of man and other animals, constant and remarkable differences are observed in them all; but as this does not immediately bear on my subject, I shall omit the consideration of all except that of man. If the foregoing experiment be made on the bile of the human subject, the smell given off on boiling is more penetrating and offensive, while the pitchy precipitate is not in general so concrete. The liquor from which it is separated differs remarkably in colour, resembling port wine in appearance. The resinous matter obtained by the preceding process is of a brighter colour; the proportion of chloride of sodium is less, and in no case have I observed taurine. These remarks only apply to bile of healthy characters, of which I consider the port wine-coloured fluid a very good criterion. In diseased bile the solid precipitate is often in the form of grains, and the fluid is like dirty water. The bile of children differs from that of adults, in having the precipitate less solid and the fluid much paler in colour.

We may now briefly examine the nature of the chemical action observed in this analysis by acids. As bile is perfectly soluble in alcohol, the first step taken, viz. the precipitation of the mucus, needs no remark. The next precipitate is the dark pitchy mass, which on analysing will be found to consist of green colouring matter, a large portion of resin, and some soda, which even the boiling with acids does not immediately remove; possibly the pitchy nature of the mass presenting a mechanical obstruction to the chemical action. The fluid portion of a purplish colour is a solution of resin discoloured by the heat employed, and does not differ in its nature, as I have satisfied myself from that at first precipitated. As the evaporation proceeds, the whole of this resin is deposited, and the use of alcohol readily separates the crystalline portion. M. Demarcay considers that the two precipitates are different, and has named the one "choléique," the other "choloidique acide." This opinion I at first adopted, but have since abandoned. If we take a portion of the first precipitate, and place it on a water-bath with concentrated acid and digest it some time, it will thus treated, assume all the characters of the second precipitate mentioned in the experiment, and a portion of chloride of sodium, and taurine when it exists, be obtained. It appears to be of little moment which acid is employed, as the chemical action of them all is the same. There is, however, an objection to sulphuric, as during the application of heat it blackens the whole mass, so as to render it obscure. It is stated by Demarcay, that acetic acid does not produce decomposition. There is, however, an immediate change of colour produced by its admixture with bile. The experiments made by Berzelius were conducted by means of this acid; and it was by its aid that his observations on the fatty acids which he termed "cholenique" and "fellique" were made. Hence, I conceive this remark of M. Demarcay to stand in need of some explanation. The taurine mentioned as obtained in the foregoing experiment is best procured from the bile of the ox, in the manner recommended by M. Demarcay. The process given by Berzelius is much less eligible. I believe it is entirely wanting in the bile of man, and I have not hitherto observed it in that of the pig. It has little taste, being neither sweet nor saline, but having a pungent flavour. It is easily dissolved by nitric acid, and, when burned in the platinum spoon, gives a large volume of carbonaceous matter. If the heat be very gradually applied, it assumes the appearance of a dark-brown oil.

The action of the salts of lead upon bile is precisely analogous to that of the acids, and the conclusions arrived at by the aid of the one may be equally obtained by the use of the other. Cadet appears to have been the first who observed that, after adding sugar of lead to bile, by which a copious precipitate is produced, there yet remains a resinous substance in solution. This substance was described by Thenard under the name of picromel, and has since been the object of attention to a great number of chemists, especially to Gmelin. Berzelius, indeed, expresses his suspicion that the resin, picromel, and biliary matter, are not distinct substances.

Experiment 2.—I added sugar of lead to ox bile, by which a copious precipitate was immediately produced. I then passed sulphuretted hydrogen gas through the mixture, filtered and evaporated on a water-bath until I had obtained the extractiform mass of Berzelius, which was sweet and bitter to the taste, and answered to the description of picromel of Gmelin. This I dissolved in alcohol, when a clear yellow fluid was observed, with a white flocculent substance at the bottom of the vessel, being that portion of lead not removed by the sulphuretted hydrogen. The yellow fluid was then evaporated to dryness, and when pulverized was nearly white, resinous in its character, bitter and sweet to the taste. During the pulverization it produced great irritation of the nares, and the lips tasted excessively bitter, as when aloes are pulverized. After some hours it had attracted such a degree of moisture from the air as to have again become nearly fluid.

In the first part of this experiment the precipitate caused by the sugar of lead is precisely similar to that caused in the former experiment by the muriatic acid, being a heterogeneous mass of colouring matters, biliary matter, and soda; that left in solution being exactly the same substance which is obtained at the latter part of the evaporation of the acid, but still in combination with oxide of lead. It is worthy of remark, that the lead is not wholly removed by the action of the sulphuretted hydrogen, nor yet entirely by the alcohol. I have already expressed my opinion, that the ordinary action of these chemical agents is modified in all probability by the animal matters existing. I have, in experiments made on this point, satisfied myself of the insufficiency of sulphuretted hydrogen to remove the lead; the solutions through which it was passed remaining colourless after its repeated employment, when, by dissolving the dried mass in strong alcohol, a large portion of lead has been separated, some even yet remaining in combination with the resin. It is this circumstance, I believe, which led Gmelin, Thenard, and others to speak of picromel as a peculiar biliary principle. Dr Fyfe observes, that it is necessary "to employ the sulphuretted hydrogen very repeatedly, and even then a small portion of lead will still remain." Demarcay complains of the same difficulty, and uses strong alcohol; but this I have shown is still insufficient. The following simple experiment suggested itself to me, which not only proves the picromel to be a nonentity, (or rather two substances in combination with each other,) but completely obviates the difficulties just described.

Experiment 3.—Having obtained, by means of the salts of lead, the usual form of picromel, or rather what is more convenient, the extractiform mass of Berzelius, I dissolved it in tepid distilled water, and added carbonate of potassa until the effervescence at first produced had ceased. As soon as the liquor had become clear, the carbonate of lead was seen lying in large quantity at the bottom of the vessel. I then carefully filtered the fluid, which had a most nauseous taste, being a solution of resin and carbonate of potassa; and evaporated it until the saline matters began to crystallize, and the mass to assume a resinous tenacity. I then kneaded the whole in weak nitric acid, and once more evaporated to dryness, and added strong alcohol, which dissolved the resin, leaving crystals of nitrate of potassa on the filter. On evaporating the alcoholic solution I obtained pure resin, if we except a trace of soda which still obstinately remained, but which may be removed by a process to be presently noticed. This experiment is perfectly satisfactory, the carbonate of potassa completely removing the lead, and the nitric acid, while it has no action on the resin, is at the same time sufficient to remove the potassa.

We have now only to prove that the picromel of Gmelin and Thenard has no solvent power, and is itself insoluble, and that the resin observed in the first precipitate by muriatic acid, and that obtained in the second part of the process, are identical with each other, the solubility of the one simply depending on the soda with which it is combined.

Take a portion of picromel, dissolve it in tepid distilled water, add *guttatim* strong nitric acid; very soon snow-white flakes are observed to form, which gradually unite, adhere to the sides of the vessel, and become slightly yellow. Filter and dry the solid matter, gently heating it when you will have obtained a perfectly insoluble resin. Evaporate next the fluid, and nitrate of soda is found remaining. Take the "azotized soluble substance" of Demarcay, and perform the same experiment, and the same will be the result. The reason that the resin first found is soluble is merely from its containing soda which the high temperature and more concentrated state of the hydrochloric acid afterwards separates. Thus, not only does the substance named picromel vanish, but also one of the principal constituents of M. Demarcay's analysis, "the soluble azotized matter." It may require some apology that I speak with so much decision on a point upon which a different opinion has been entertained by men so eminent as Thenard, Gmelin, Chevreul, Chevalier, Lassaigne, Tiedemann, Fromhertz, Gugert, and others; but the simple experiments I have detailed do not appear to me to admit of doubt, and remain open for investigation by those who may not feel inclined to admit the views I have entertained.

I have pointed out in the commencement of my paper the necessity there is of arriving at some general analysis of which the pathologist may avail himself, in order to form any certain conclusions as to the healthy or morbid state of the biliary secretion. I beg to offer in conclusion, the following analysis, which has been the result of my own experience; but to which I was led by the perusal of M. Demarcay's excellent paper, and which appears to me to meet the object in view. It is calculated for the examination of the contents of the gall-bladder after death.

Analysis of the contents of the Gall-Bladder.—Empty the contents of the gall-bladder into a measure glass, (this is best done by dissecting out the gall-bladder,) and, having noted the quantity, dilute it if requisite until it be limpid. Add to this sufficient alcohol to precipitate the whole of the mucus, which, being separated, next add concentrated muriatic acid, stir it assiduously for some time, and leave it for an hour, when, if there be any cholesterine, it will be seen floating in thin scales upon the surface. This may be elevated by the aid of a piece of blotting-paper. Next place the whole in a flask, and boil gradually for an hour and a-half to two hours. A blackish mass and a port-wine coloured fluid will be obtained. Separate the two, evaporate the fluid until the resin with which it is coloured is deposited together with the chloride of sodium. If it be desirable to make the analysis very complete, it will now be advisable to add very strong alcohol. This dissolves the resin, purifies the chloride of sodium, and allows any other saline matter that may be present to crystallize. Next dissolve the black mass first obtained in warm distilled water. A milky fluid of a greenish white colour is produced. Allow this to remain some hours, when an insoluble dark-green matter will be seen to have separated at the bottom of the vessel. Pour off the milky fluid, and wash the insoluble green matter in tepid distilled water, adding the washings to the rest of the milky fluid. To the latter next add strong nitric or muriatic acid (muriatic will be the best) until the opaque fluid is perfectly clear, and there is a precipitate of a resinous matter. This precipitate is to be washed, dried on a water bath, and added to the resin first obtained. If it be desirable to proceed with accuracy, the fluid from which the second portion of resin was obtained should be evaporated, by which a small quantity of chloride of sodium will probably be obtained, which is to be added to the portion first procured.

We have thus obtained the relative proportions of the four following substances. 1. a dark green insoluble matter; 2. resin; 3. cholesterine; 4. chloride of sodium and other salts.

There is no objection in this analysis to the use of the ordinary muriatic acid, which contains a certain proportion of sulphuric acid, as the action of all these acids is the same as regards the bile; and if, as is usually the case, the bile contains a portion of phosphate or carbonate of lime, it is decomposed by the sulphuric acid, and the sulphate thus formed from its insolubility is very easily recognized.

By performing this analysis a few times with healthy bile, a mean would be very easily obtained of the relative proportions of these substances, from which comparisons might as easily be made in cases of disease. The process is simple, yet sufficient; and a reference to the former part of my paper will show how much more valuable the remarks of Andral, Bright, Annesley, and others would have been, had they been the result of some such definite examination as that I have now suggested.

When I first turned my attention to this subject, I looked in vain through the different chemical works for some such analysis as that I have given for my guide. It would indeed have been of the utmost value; and I have therefore endeavoured to afford to others who may pursue this line of investigation that assistance which was necessarily denied to myself.

ART. II.—*Pathological Contributions.* By Dr BOYD, Resident Physician, St Marylebone Infirmary.

The following Table is intended to show the *post mortem* appearances presented in 75 cases, of which 28 were males, and 47 were females, examined at the St Marylebone Infirmary, during the autumn and close of the year 1839. The 75 cases were distributed as follows :—

1st. *Digestive Organs.*—Ulcer and perforation of stomach, 1 ; cancer of stomach, 1 ; icterus, 2 ; diarrhœa, 2 ; dysentery, 1 ; peritonitis, 1, ...	8
2d. *Respiratory Organs.*—Suffocation, 1 ; pulmonary apoplexy, 1 ; bronchitis, 8 ; pneumonia, 4 ; phthisis, 15 ; pleurisy, 2, ...	31
3d. *Circulatory Organs.*—Enlarged heart, 5 ; rupture of aorta, 1, ...	6
4th. *Genito-Urinary Organs.*—Granular kidney, 1 ; dropsy, 1 ; cancer, 3, ...	5
5th. *Locomotive Organs and Cellular Tissue.*—Gangrene, 1, ...	1
6th. *Nervous Organs.*—Hydrocephalus. 1 ; apoplexy, 7 ; paralysis, 3 ; tumour on brain, 1 ; ossific deposit on *dura mater*, 1, ...	13
7th. *Skin.*—Burn, 1 ; erysipelas, 1 ; measles, 1, .	3
8th. *Affecting System generally.*—Typhus, 3, .	3
9th. *Not included in the above.*—Premature births, 5,	5
Total, .	75

These cases have been classified for the purpose of showing the complications, together with the structural and organic changes in each disease. The method of examination pursued at the infirmary is distinguished from that employed in other hospitals in one essential point,—that of weighing not only the body but each particular organ.

For the purposes of instituting comparisons, it was therefore necessary to take a healthy standard of organs; and this has been obtained from the Croonian Lectures for 1838, delivered by Dr Clendinning, and published in the Medical Gazette and Farr's Medical Almanack for 1839. It is as follows, giving the average absolute weight for the whole of life above puberty, in ounces Avoirdupois.

Organs of the Male.—Brain, 45½ ounces ; lungs, 46½ ; heart, 9 ; liver, 49 ; spleen, 4¾ ; stomach, 4½ ; pancreas, 2¼ ; kidneys, 8⅓.

Organs of the Female.—Brain, 42 ounces ; lungs, 35½ ; heart, 8⅛ ; liver, 44½ ; spleen, 4½ ; stomach, 4⅔ ; pancreas, 2¼ ; *kidneys, 8.*

DISEASES OF THE DIGESTIVE ORGANS.

CASE I.—Female, aged 42. Ulceration and perforation of stomach. Had been twice salivated from two or three grains of calomel. Upon the last occasion, it was in combination with jalap.

Appearances.—A large ulcer at the pyloric extremity of stomach, freely exposing the head of the pancreas. Omentum adherent around it; no thickening of the edges, or other appearance that would mark the ulcer as a malignant one. Liver only 31 ounces, and adherent to the stomach through the medium of a fibrous membrane a quarter of an inch thick.

CASE II.—Female, aged 63. Cancer of cardiac extremity of stomach; vomiting very frequent, latterly with blood.

Appearances.—*Thorax.*—Muco-purulent secretion from the membranous lining of the bronchial tubes. Heart below average weight, 7¼ ounces.

Abdomen.—Ulcerated opening with thickened edges in cardiac extremity of stomach. The liver weighed 40 ounces; kidneys, 6.

CASE III.—Female, aged 71. Jaundice, complicated with emphysema of lungs.

Appearances.—*Head.*—*Dura mater* of a deep-yellow colour, in common with the other fibrous tissues throughout the body.

Thorax.—Both lungs emphysematous, margins especially.

Abdomen.—Preternatural hardness of liver. Gall-bladder filled with calculi.

CASE IV.—Female, aged 58. Blind of left eye; jaundice complicated with dropsy, bronchitis, and emphysema of lungs.

Appearances.—*Head.*—*Dura mater* yellow. Left optic nerve smaller than right.

Neck.—A tumour on left side of thyroid body, size of a duck's egg, consisting of three or four separate parts, in one capsule, extending toward sternum. A bony cyst in its centre was divided by a saw, and found to contain about a drachm of glairy fluid.

Thorax.—Muco-purulent secretion from bronchial membrane; emphysema of lungs, and particularly the thin edges anteriorly; heart enlarged; weight 11¼ ounces.

Abdomen.—Liver very firm; colour of a nutmeg; weight 37 ounces.

CASE V.—Male, aged 73. Diarrhœa, complicated with paralysis of right side; vital powers for some days before death extremely low.

Appearances.—*Head.*—Tumour in left hemisphere of brain.

Thorax.—Heart enlarged, weighing 13 ounces. A globular clot of coagulated fibrine in left auricle, size of a pigeon's egg, firmly adherent to the auricular septum, and covered by a membrane apparently continuous with the endocardium; the clot was injected from the coronary artery; *foramen ovale* open, so as to admit a probe. Specimen prepared.

Abdomen.—No recorded cause of diarrhœa. Other organs normal.

Case VI.—Female, aged 66. Diarrhœa, combined with vomiting. Nothing abnormal but the heart, which weighed 11½ ounces.

Case VII.—Male, aged 66. Dysentery of long continuance.

Appearances—Thorax.—Heart enlarged, weighing 14 ounces.

Abdomen.—Ulceration of many parts of the mucous membrane of the cœcum and colon, with general thinning of large intestines. Bony depositions in three or four places in the mesentery; largest one size of a marble. Capsule of spleen semicartilaginous.

Kidneys.—Cysts on surface containing a muddy fluid.

Case VIII.—Male, aged 20. Peritonitis, with œdema of left leg and thigh.

Appearances.—Thorax.—Lungs very slightly diseased.

Abdomen.—Tuberculous depositions on peritoneum, with effusion in sac. A plug of coagulated fibrin in femoral vein of left side; plug not adherent; softening in the centre.

Diseases of Respiratory Organs.

Case IX.—Female child, newly born, weighing 6 lb. 2 oz. height 21 inches, on which a coroner's inquest was held. The birth was concealed. Suffocation from natural causes. The child was found in a box, rolled up in a piece of brown silk.

Appearances.—Decomposition commencing about the eyes, lips, nose, and umbilicus. The cord two inches long, not tied; centre of body about half an inch above the navel, the entire length of body being measured. A slight mark on forehead. Soft palate greenish colour. Œsophagus clean. Congestion of the vessels of the brain; weight of brain 8½ ounces.

Thorax.—Lungs in close contact with the ribs, light in colour; floated lightly; fully dilated with air. A dark-grayish substance was found in larynx, which was too large to pass through the *rima glottidis*, and a smaller portion of same substance lower down at opening of left bronchus. Pulmonary vessels filled with blood. *Ductus arteriosus* open; lungs weighed 1¼ ounce. No blood in the heart; weight ¾ ounce.

Abdomen.—Intestines stained with bile; glairy mucus in small intestines; large ones filled with meconium. Bladder filled with urine.

Case X.—Male, aged 58. Pulmonary apoplexy combined with pneumonia, pleuritis, bronchitis, hypertrophy of the heart, and pericarditis.

Prominent Symptoms.—Pulse feeble and irregular; dyspnœa and faintness; sickness and vomiting occasionally. Died three weeks after admission.

Appearances—Thorax.—Pleura on right side filled with fluid. The lung on that side adherent superiorly; pneumonia of inferior lobe; part of it infiltrated with blood. Lymph effused on surface posteriorly, which was easily detached. Bronchial lining of both lungs congested and thickened; upper lobe of left lung also adherent, slightly emphysematous, and very dry at anterior edge over the pericardium. Left lung weighed 21½, right 42 ounces. A hard tumour, size of a walnut, at root of left lung, pressing on *par vagum*, and connected with left bronchus and œsophagus.

Pericardium closely adherent to the heart, which was enlarged, weighing 21½ ounces.

Abdomen.—Right renal capsule's situation occupied by a fibro-cartilaginous structure. Left normal.

Case XI.—Male, aged 70. Bronchitis, complicated with hypertrophy of the heart, hydrothorax, and pulmonary apoplexy.

Appearances—Thorax.—Some fluid in chest; bronchial membrane thickened; tubes nearly filled with muco-purulent secretion; some dark spots (effused blood) in both lungs, which, when cut out with some of the surrounding structures, sank in water. Enlargement with dilatation of heart; weight 17 ounces.

Abdomen.—Organs enlarged; liver, 48 ounces; stomach, 7; pancreas, 3½; kidneys, 12; spleen, 9½; capsule partly cartilaginous.

Case XII.—Male, aged 60. Bronchitis, complicated with emphysema of lungs; hypertrophy of the heart; and anasarca.

Appearances.—Thorax.—Some fluid in right pleura; thickening of the bronchial membrane, with muco-purulent secretions. Emphysema of their edges, and lower portion of lungs.

Hypertrophy, with dilatation of the cavities of the heart; weight 15 ounces; a clot of fibrin in left ventricle; ossific deposit in arch of the aorta.

Abdomen.—Liver enlarged, weighing 54 ounces. Kidneys granular.

Case XIII.—Female, aged 30. Bronchitis, complicated with emphysema, and ascites.

Appearances—Thorax.—Lungs presented the usual appearances, as already described in similar cases. Heart enlarged, weighing 12¼ ounces.

Abdomen.—Kidneys very much enlarged, weighing 17 ounces.

Case XIV.—Female, aged 92. Bronchitis, with emphysema.

Appearances.—Brain-vessels much congested.

Thorax.—Usual appearances in the lungs. Hypertrophy of the left ventricle of the heart; weight 12 ounces.

Abdomen.—Ossification to a great extent of the coats of the splenic and iliac arteries. Cartilaginous deposit in renal arteries. Congestion of kidneys.

Case XV.—Female, aged 25. Acute bronchitis, following an attack of measles, twelve days after confinement. Eruption livid and general; pulse very feeble; great dyspnœa. The woman suckled a child with measles.

Appearances.—Thorax.—Lining membrane of bronchial tubes very red; lungs generally emphysematous; heart enlarged; right auricle and ventricle filled with a coagulum; weight 11¼ ounces.

Abdomen.—Uterus soft at fundus, rough internally; weight 4¼ ounces, four weeks after delivery. Liver enlarged, weighing 83½ ounces.

Case XVI.—Female, aged 64. Bronchitis, with emphysema.

Appearances.—Thorax.—Lungs presented the usual appearances in the chronic form of this disease. Heart weighed 9¼ ounces.

Abdomen.—Thinning of the coats of the intestines.

Case XVII.—Female, aged 44. Bronchitis and emphysema complicated with renal disease.

Appearanees.—Thorax.—Lining membrane of bronchial tubes thickened, and lungs at lower surface resting on diaphragm, presented large bullæ, and were preserved as being a well-marked specimen of emphysema. Heart weighed 10 ounces.

Abdomen.—Congestion of liver. Right kidney contained a pulpy substance, and was atrophied, weighing but 3¼ ounces; left one weighed 5½ ounces.

Case XVIII.—Female, aged 89. Emphysema of lungs, complicated with hypertrophy of the heart. The symptoms were not urgent. She seemed to die of old age.

Appearances.—Head.—Brain rather soft.

Thorax.—Upper lobes and anterior edge of lower lobes emphysematous. Heart enlarged, with dilatation of all the cavities, which were filled with coagulated blood, weight 14½ ounces; valves puckered and thickened, more especially the mitral; ossific depositions in descending aorta.

Note.—Out of the eight recorded cases of asthma seven were complicated with enlargement of the heart. The last case of dry asthma, or emphysema without bronchitis, is one in which it may be questioned whether the condition of lung presented is a pathological one, or physiological, dependent on age. The result of our inspections here would rather lead us towards the latter opinion.

Case XIX.—Female child, aged two months. Pneumonia.

Appearances.—Thorax.—Lobes of both lungs solid.

Case XX.—Female child aged one month; weight 72½ ounces. Pneumonia occurring after measles.

Appearances.—Thorax.—Left lung and lower lobe of right solid. Weight of both 2¾ ounces.

Case XXI.—Female, aged 81. Pneumonia, complicated with pleuritis, bronchitis, and emphysema.

Appearances.—Head.—A considerable quantity of fluid between the convolutions and in the ventricles of the brain.

Thorax.—Pneumonia in the gray softened state in the superior lobe of left lung. The inferior lobe of right lung in the primary stage of pneumonia. Lymph effused on pleura, apparently of recent origin. No pleuritic adhesions. A quantity of mucopurulent secretions in bronchial tubes, with thickening of their lining membrane. Those portions of lung not affected with pneumonia were emphysematous.

Case XXII.—Male, aged 46. Pneumonia, with some effusion in right side of chest.

Appearances.—Head.—Fluid in lateral ventricles of brain.

Thorax.—Both lungs diseased. Pleuritic adhesions of right side (easily detached) with some effusion of fluid. Upper lobe of right lung hepatized; lower lobe sunk in water. One-third of left lung in a state of hepatization.

Heart enlarged, weighing 15 ounces.

Abdomen.—Liver large. Right kidney diseased.

Case XXII—XXXVII.—Analysis of fifteen cases of *phthisis*, of which nine were males, and six females. Emaciation in every case but one, and that was complicated with pneumothorax, which was the immediate cause of death.

Appearances.—Head.—In two cases, which were attended with delirium, there was a considerable quantity of fluid in the lateral ventricles of the brain,—in one 3 ounces,—accompanied with softening of the *septum lucidum.*

Thorax.—In five cases, tuberculous cavities were found in both lungs. In five cases the cavities were confined to the left lung, and in four to the right. In all these cases the cavities were in the superior lobes. One case, in a child eight months old, there were tubercles in an advanced state in the left lung, and pneumonia in the right. In one case the lung became gangrenous ten days before death. Breath very offensive; odour perceptible on entering the ward. The lungs heavier than natural, weighing from 50 to 80 ounces. Pleuritic adhesions in all, and in one effusion into pleura. Hæmoptysis in two cases. The heart in one case weighed 11 ounces, and in two others 10 ounces each. In five cases it was below the average weight. In one case the heart and pericardium were adherent.

Abdomen.—Inflammation of peritonæum in one case. One case of tympanitis. Serous cysts or hydatids, varying in size from a walnut to a swan-shot, and amounting to six or eight ounces, found in the cavity of the pelvis of a male. In another case of a male, aged 23 years, there were five invaginations in the course of the ileum.

Three cases of diarrhœa with ulceration of the mucous membrane of the intestines.

Spleen.—In one case double the average weight; in two cases below it. In two cases its capsule was dotted with white specks.

Extremities.—Scrofulous ulceration and caries of humerus in one case, and in one, erysipelas of right leg.

CASE XXXVIII.—Male, aged 52. Pleurisy; effusion of fluid in right side of chest, complicated with a very large irreducible scrotal hernia, which was painful, attended with vomiting, sickness, and great debility.

Appearances.—Thorax.—Both lungs engorged with blood. Right lung enlarged, overlapping the pericardium; weighing 20 ounces. Left lung bound down to spine; pleura thickened; that side of chest filled with fluid. Left lung collapsed. Weighed 7½ ounces; heart weighed but 6 ounces.

Abdomen.—Hernial sac weighed 59½ ounces, and contained portions of both large and small intestines. No strangulation. A portion of small intestines very vascular.

CASE XXXIX.—Female, aged 42. Pleurisy.

Syphilitic cachexia.

Appearances.—Head.—A pustular eruption on face and scalp. Cartilages of nose destroyed. Skull very brittle, easily removed from *dura mater*; yellow fluid beneath the arachnoid of a deep colour.

Thorax.—Right lung occupied a large space; weighed 16¼ ounces. Left lung bound down to spine, much fluid, with flakes of lymph effused on pleura, and lymph on surface, which appeared to be quite recent. Lung weighed 8¾ ounces; heart 7¼ ounces.

Abdomen.—Liver adherent to diaphragm; peritoneal coat thickened, and adherent to stomach and colon, below; weight 46 ounces. A coating of lymph on upper surface of spleen similar to that on left lung; weight of spleen 7 ounces.

Kidneys.—Large, two or three cysts filled with a dark grumous fluid in the upper portion of the left; fat of a dark rusty colour.

DISEASES OF THE CIRCULATING ORGANS.

CASE XL.—Male, aged 54. Hypertrophy of the heart complicated with hydrothorax.

Appearances.—Thorax.—A quantity of fluid in pleura. Both lungs compressed; fluid also in pericardium. Heart very much enlarged, weighing 30 ounces, principally left ventricle.

Abdomen.—Liver and spleen enlarged, the former weighing 62 ounces, the latter 16 ounces. Kidneys weighed 8 ounces.

CASE XLI.—Male, aged 43. Hypertrophy of the heart with valvular disease. Pericarditis and effusion into right pleura, which was tapped three times, and 16 pints drawn off, to relieve the dyspnœa; feeble and irregular pulse.

Appearances.—Head.—About one ounce of fluid in lateral ventricles of brain.

Thorax.—Pericardium adhering to the heart, which was enlarged, weighing 21 ounces; mitral valves thickened; unfit for their office; could not close the opening. Right lung bound down to spine. Some fluid pressing on it. Pleura thickened, and bands crossing that side of chest, so that the fluid appeared to be in sacs. Some of these did not communicate with each other.

CASE XLII.—Male, aged 39. Hypertrophy of the heart complicated with emphysema. Face of a deep purple colour; subject to cough and dyspnœa. Twenty years since he had the hooping-cough. For last seven years has been much inconvenienced from pain in left side and palpitation of the heart, and is unable to lie on side. About the same time the blueness of the lips commenced, and it has increased very much of late. He wished to die at home, and left the Infirmary the day before his death. The chest only was examined.

Appearances.—Both lungs adherent about their centre to parietes of the thorax, emphysematous and engorged with blood. Hypertrophy with dilatation of heart; cavities filled with blood; *foramen ovale* sufficiently open to easily admit the handle of a scalpel.

CASE XLIII.—Male. Hypertrophy of the heart with hydrothorax, bronchitis, and emphysema.

Appearances—Thorax.—Fluid in left pleura; left lower lobe compressed with considerable congestion; upper right lobe emphysematous. Lungs 5½ ounces weight. Bronchial membrane thickened.

Heart.—Enlarged, weighing 16¾ ounces; coagula in its cavities.

Abdomen.—Old peritoneal attachments to liver and spleen. Stomach enlarged, weighing 7¼ ounces. Its veins, as well as those of the great omentum, large and tortuous.

CASE XLIV.—Female, aged 71. Hypertrophy of the heart, with irregular communication between the auricles; chronic bronchitis and general dropsy; no blueness of face in this case as in the case of the man before-mentioned. Anasarca decreased considerably before her death.

Appearances—Thorax.—A quantity of fluid in both pleuræ; lower lobes of lungs compressed; bronchial lining membrane red and increased in thickness. Lungs weighed 22 ounces. Heart enlarged, weighing 17 ounces, with dilatation of its cavities. Through the centre of their membranes anterior to the *foramen ovale* there is a round opening as if punched out, 4 or 5 lines in diameter. Cavities of the heart filled with coagula.

Abdomen.—Kidneys enlarged, weighing 12 ounces; numerous dropsical cysts on the surface of both.

Case XLV.—Male, aged 64. Hypertrophy of the heart, with hydrothorax and granular kidney. Seized on a sudden with a great dyspnœa; died within twelve hours.

Appearances—Head.—Brain normal.

Thorax.—About 5 pints of fluid in chest. Congestion of the lungs, weight 60 ounces. Left ventricle of the heart very much thickened; weight 18 ounces. An elongated osseous deposit at the root of mitral valve. Bronchial membrane thickened.

Abdomen.—Old peritonitic adhesions above pubis. Kidneys granular and atrophied, weighing 6 ounces.

Case XLVI.—Male, aged 60. Rupture of aorta in a tall athletic man, formerly a porter at the docks, where he fractured his thigh two years ago. Seized suddenly with pain and tightness of chest; very faint; pulse scarcely perceptible at wrist ten minutes after the seizure. Died in twelve hours.

Appearances.—Thorax.—Pericardium filled with coagulated blood. Heart enlarged; no valvular disease; aorta thickened, with patches of ossific deposits anteriorly. Just above the semilunar valve there was a slit through the internal and middle coats. The slits formed a T-shaped opening three-quarters of an inch long and half-an inch in breadth; a coagula of blood twelve inches long lessening the calibre of the aorta one-third, lay between the outer and middle coats of the artery.

The other organs of the body normal.

Diseases of the Genito-urinary Organs.

Case XLVII.—Female, aged 57. Granular kidneys with hydrothorax and ascites, for which she was tapped a few days before death. The fluid kept oozing constantly through the opening.

Appearances.—Thorax.—Contained about three pints of fluid. Lower lobes of both lungs congested; weight 49 ounces. Hypertrophy of the left ventricle of the heart; weight 11 ounces.

Abdomen contained but a small quantity of fluid. Kidneys granular and atrophied, weighing but 4 ounces. Liver 33 ounces.

Case XLVIII.—Male, aged 64. Cancer of the prostate and bladder; bladder very irritable; urine thick; clots of blood frequently passed from urethra; great pain; great emaciation.

Appearances.—Head.—White spots on arachnoid, and opacity of that membrane.

Abdomen.—Cancerous tubercles on convex surface of the liver beneath its peritoneal coat. Ureters dilated, as well as the pelvis of the kidneys, which were both wasted; the left one more than the right.

A tumour, firm in structure, branched out from prostate and neck of the bladder, occupying about half that organ. Some large veins on surface of diseased mass ; some thick urine in fundus of the bladder. Urethra vascular, but no stricture. Some of the lymphatic glands about the neck of the bladder enlarged, also several of the glands of mesentery.

Case XLIX.—Female, aged 55. Cancer of uterus and vagina.

Appearances.—Thorax.—Lungs weighed 24½ ounces.

Abdomen.—Liver enlarged, weighing 56 ounces. Intestines firmly adherent in the pelvis, particularly to the fundus of the uterus, which was ulcerated, opening into peritoneum. *Os uteri* and upper portion of vagina ulcerated. Atrophy of right kidney, weight 2½ ounces ; left one enlarged, weighing 8½ ounces.

Case L.—Female, aged 23, married. Cancer affecting external parts of generation, right groin, and rectum. The labium of right side removed by operation, disease returned, and the parts above-mentioned became diseased.

Appearances.—Thorax.—Lungs weighed 28 ounces ; heart 6¼.

Abdomen.—Liver enlarged and fatty, weighing 60 ounces. Kidneys pale, weighing 9 ounces. Uterus free from disease. Lower portions of the vagina, rectum, and surrounding parts ulcerated, ragged, and indurated. A large cancerous mass in right groin. Ramus of the *os pubis* was softened, and easily divided with a scalpel.

Diseases of the Locomotive Organs.

Case. LI.—Female, aged 94. Gangrene of right foot. Discharged from Infirmary six months before, having lost the great toe of right foot by gangrene.

Appearances.—General emaciation and atrophy of the organs, except the heart, which weighed 9½ ounces.

The remaining four toes and dorsum of foot in a gangrenous state. Slight discoloration of outer ankle, and a spot on the leg. Posterior tibial artery blocked up by a firm fibrinous chord. Ossific depositions in the aorta and right femoral artery.

Diseases of the Nervous Organs.

Case LII.—Male, aged 73 ; full habit, and a great eater ; serous effusion into the ventricles of the brain, with hypertrophy of the heart.

Appearances.—Head.—Opacity and thickening of the arachnoid membrane ; a large quantity of fluid in the lateral ventricles. Brain weighed 50½ ounces.

Thorax.—Congestion of lungs, weight 61 ounces. Ossific deposits in the bronchial tubes. Heart enlarged, weighing 15 ounces. Ossific deposits in arteries.

Abdomen.—Cartilaginous deposits on surface of spleen ; spleen weighed 5½ ounces. Kidneys enlarged, weighing 10½ ounces. Lining membrane of bladder thickened.

Case LIII.—Male, aged 65. Apoplexy, stupor; no paralysis.

Appearances.—Head.—Blood effused on *dura mater* at each side and base of the skull; arachnoid opalescent; a considerable quantity of fluid on surface of brain; several small serous cysts throughout the substance of the brain; absorption had already taken place in *corpora striata*, notwithstanding the brain weighed 45¾ ounces.

Thorax.—Congestion of lungs, weight 55½ ounces; left ventricle of the heart thickened, weight 11¼ ounces.

Abdomen.—Atrophy of the organs. Liver weighing 35¼ ounces; spleen 2½ ounces; and kidneys 5 ounces.

Case LIV.—Male, aged 68. Apoplexy, followed by gangrene of brain; speechless and quite powerless from time of his seizure to his death, seven days afterwards. Body examined twenty-seven hours after death.

Appearances.—Head.—Blood appeared to have been poured into the right hemisphere about its centre; four-fifths of hemisphere in a disorganized state. The blood was also infiltrated into the substance of the right *corpus striatum*, giving it a mottled appearance. The inferior portion of the left hemisphere green and soft, with an unpleasant odour. Cerebellum normal.

Thorax.—Old pleuritic adhesions and congestion of lungs; weight 36 ounces. Heart weighed 11 ounces.

Abdomen.—Firm adhesions between intestines and parietes in several places. Congestion of mucous membrane of stomach; weight four ounces. Left kidney granular, and a much larger renal capsule on that side than on the right; weight 3 ounces.

Case LV.—Female, aged 47, with serous effusion into the lateral ventricles; no paralysis; hypertrophy of the heart.

Appearances.—Head.—Convolutions of the brain flattened; about three ounces of fluid in lateral ventricles; a large clot of blood in the left lobe of cerebellum; brain, 44¼ ounces.

Thorax.—Congestion of left lung. Weight of both lungs 33 ounces. Hypertrophy of left ventricle of the heart; weight 17 ounces.

Abdomen.—Some of the organs enlarged. Stomach weighed 6½ ounces; liver, 55 ounces; spleen, 4¾ ounces.

Case LVI.—Female, aged 61. Apoplexy without effusion or paralysis.

Appearances—Head.—Great congestion of the vessels of encephalon.

Abdomen.—Liver and diaphragm adhering. Gall-bladder very small, about the size of a garden bean. Weight of liver 40½ ounces. Kidneys enlarged, weighing 12 ounces. Ossification of abdominal aortà extending from the renal arteries to its bifurcation. Spleen soft and large; weighed 6 ounces A polypus, size of the little finger, attached to neck of uterus.

Case LVII.—Female, aged 70. Apoplexy with paralysis of the right side, and disease of the lungs.

Appearances.—*Head.*—An ossific deposition, size of the palm of the hand, on the *dura mater*, on the side of left ventricle of the brain; congestion of the vessels, and a considerable quantity of fluid in lateral ventricles of brain.

Thorax.—Cartilages of the ribs cut readily with a scalpel. Emphysematous state of the thin edges of the lungs, with several spots of gray hepatization in both of these organs. Left ventricle of the heart thickened, weighing 9½ ounces.

Abdomen.—Capsule of spleen semicartilaginous. Both kidneys atrophied, weighing 5 ounces.

Case LVIII.—Female, aged 43. Apoplexy, with effusion of blood into both lungs, and enlargement of the heart. Died three days after having had a fit; insensible the whole of that time.

Appearances.—*Head*—A clot of blood on external surface of right hemisphere, and between the convolutions. A fissure in left *corpus striatum* external to its white fibres, which appeared to be of long standing. Weight of brain 40¾ ounces.

Thorax.—Blood effused on both lungs; weight 34½ ounces. Heart enlarged, weighing 13 ounces.

Abdomen.—Spleen weighed 6½ ounces; kidneys 10 ounces.

Case LIX.—Female, aged 44. Apoplexy, with effusion into lateral ventricles, and softening of brain; emaciation of body; and general atrophy of organs.

Appearances.—*Head.*—An apoplectic clot of a brown colour encysted on the outer side of left hemisphere; softening of the substance of surrounding brain; some fluid in the lateral ventricles; weight of brain, 35 ounces.

Thorax.—Lungs weighed 25 ounces; heart, 7½ ounces.

Abdomen.—Liver weighed 43½ ounces; spleen, 2½ ounces; and kidneys, 7 ounces.

Case LX.—Female, aged 40. Tumour in the brain. Contraction of the lower extremities, and loss of power in the sphincters. For several weeks before death, the nasal and part of the malar bones of the left side were destroyed by caries from syphilis; the parts were firmly cicatrized over slough on sacrum. Great emaciation of the body.

Appearances.—*Head.*—The *dura mater* adhering firmly to skull over a space about the size of a penny-piece on the upper and posterior portion of the right hemisphere. The brain beneath that spot, to the depth of three-quarters of an inch, very firm and changed in structure, like to carcinoma. External to this, still deeper, there was a cluster of small cysts, containing a serous fluid, forming together a mass the size of a pigeon's egg. Substance of brain absorbed. On the external inferior portion of the left hemisphere there was a similar appearance, but much smaller than that on the right; atrophy of the brain. Weight 27½ ounces.

Thorax.—Heart weighed 7¼ ounces.

Abdomen.—Liver enlarged; weighing 56 ounces; spleen, 5½ ounces.

Case LXI.—Female, aged 63. Chronic disease of brain occurring in a nurse addicted to drunkenness. On admission, her symptoms were those of *delirium tremens*, and debility, which gradually increased.

Appearances.—Head.—Ossific growth from inner table of frontal bone, and also between the layers of *dura mater* at the anterior portion of longitudinal sinus. Atrophy of the gray matter of the right *corpus striatum;* no paralysis. Brain weighed 38½ ounces.

Thorax.—Lining membrane of bronchial tubes thickened; muco-purulent secretion. Weight of lungs, 40 ounces. A quantity of fat about the heart, which weighed 9½ ounces.

Abdomen.—Enlarged fatty liver, weighing 70 ounces; spleen weighed 6 ounces.

Case LXII.—Female, aged 52. Idiotic; paralysis of left lower extremity with contraction; heel resting on nates, and knee drawn up in a line with the clavicle. Any attempt to straighten gave pain. The contraction succeeded a fit of convulsions about a month prior to her death. The idiotcy and paralysis of more than one year's standing.

Appearances.—Head—Numerous cavities filled with clear fluid, and lined by a membrane in white substance of both hemispheres of the brain. The cavities varied in size from a millet-seed to that of a hemp-seed. One large cavity nearly the size of an almond filled with fluid, and lined by a membrane of a rusty colour.

Both lateral ventricles dilated with fluid. Brain weighed 41¾ ounces.

Thorax.—Lungs weighed 32¾ ounces; heart 7½ ounces.

Abdomen.—Mesenteric glands towards cardiac end of stomach enlarged, containing matter. Right renal capsule darker and softer internally than left one. Liver atrophied, weighing 32 ounces; kidneys, 7½.

Case LXIII.—Female, aged 77. Hemiplegia of one side.

Appearances.—Head—Opacity of arachnoid membrane; a considerable quantity of subjacent fluid. Softening of the *centrum ovale* of right side. Atrophy of brain, which weighed 39 ounces. Other organs normal.

Case LXIV.—Female, aged 35. Idiotcy; paralysis of both lower extremities; contraction and loss of powers of sphincters; very noisy occasionally during the last five months.

Appearances.—Head.—A large prominent frontal bone. Arachnoid membrane thickened and opaque, with a considerable quantity of subjacent fluid. Brain small; weighing 33¼ ounces. No other organic lesion. Slough on sacrum.

Diseases of the Skin.

Case LXV.—Female, aged 89. Burn on the front of the neck, chest, and abdomen. Death followed almost immediately.

Appearances.—Head.—A thin layer of effused blood on the arachnoid membrane over the posterior and superior third of the right hemisphere of the brain. Fluid in the lateral ventricles and spinal canal. The brain weighed 33½ ounces.

Thorax.—An obvious difference betwixt the two sides of the chest. Right side contracted. Effusion of fluid into right pleura; the lung bound down to spine; pleura thickened; old pleuritic adhesions. Ossific deposits in the bronchial tubes and aorta.

Abdomen.—Slight congestion of liver; no congestion of the intestinal mucous membrane. Abdominal organs small. Liver weighed 32½ ounces; spleen, 1½ ounce; kidneys, 6 ounces; stomach, normal, 4½ ounces.

Case LXVI.—Female, aged 55. Erysipelas of the face, coma, difficulty of articulation, and paralysis of the left arm of several months standing; she was likewise subject to convulsions.

Appearances.—Head.—Congestion of the encephalic vessels; a spot of softening, the size of a split bean, in the white portion of the left hemisphere just anterior to the *corpus striatum.*

Thorax.—Heart enlarged, weighing 15¼ ounces; lungs, 38½ ounces.

Abdomen.—Liver and spleen enlarged; the one weighing 61 ounces, the other 12½ ounces. Kidneys atrophied, weighing 6½ ounces. A large cyst filled with a straw-coloured fluid, which weighed 65½ ounces, was attached to the extremity of the Fallopian tube of left side, and, having no other attachment, was quite loose in the abdomen. It had the appearance of a distended bladder, and, being in that situation, was mistaken for it at first.

Case LXVII.—Female infant, 3 weeks old. *Rubeola nigra.* Died the day after the appearance of the eruption in convulsions.

Appearances.—Head.—Congestion of the vessels; no fluid in the lateral ventricles of the brain.

Thorax.—Weight of the lungs, 2½ ounces; normal; all the cavities of the heart gorged with blood; weight, ¾ ounce.

Diseases affecting the System generally.

Case LXVIII.—Male, aged 62. Typhous fever with bronchitis. The affection of his chest was not evident till the third day after admission. He died on the sixth day after the attack.

Appearances.—Head.—Brain normal, weighing 46 ounces.

Thorax.—A deep depression on the sternum at the junction of the ensiform cartilage. Redness and thickening of lining membrane of bronchial tubes, and congestion of lower lobes of both lungs. Heart enlarged, weighing 11½ ounces; colourless fibrinous spots in its cavities, and also in the aorta.

Abdomen.—About twelve inches of the ileum quite transparent and very thin; no softening or ulceration of its coats. Kidneys pale, weighing 8½ ounces; serous cysts on the surface of both, and two or three in the substance of the right. Exostosis of all the lumbar and three last dorsal vertebræ; ribs hard and smooth, as if covered with enamel. No curvature of the spine.

CASE LXIX.—Female, aged 73. Typhus, complicated with chronic bronchitis.

Appearances.—Head.—Some fluid in lateral ventricles of the brain.

Thorax.—Thickening of the bronchial lining membrane; tubes filled with muco-purulent secretion. Old adhesions of pericardium to the heart; weight, 10½ ounces. Ossific depositions in the coats of the internal carotids, at their origins, as well as at the sides of the *sella turcica.*

Abdomen.—Spleen soft; weight, 4 ounces. Atrophy of the kidneys; weight, 6 ounces.

CASE LXX.—Female, aged 19. Maculated fever; three weeks duration; great cerebral excitement.

Appearances.—Head.—Brain very large and firm, weighing 46¾ ounces.

Thorax.—Congestion of lower lobes of both lungs; weight, 45 ounces. Dilatation of the cavities of the heart; weight, 9¾ ounces.

Abdomen.—The pelvis of the right kidney contained a brownish deposit, and was very much congested. The internal surface of bladder and vagina very red.

CASE LXXI.—Female, aged 12 days. Premature birth; weight, 3½ pounds; unable to draw its mother's breast, apparently from its great weakness.

Appearances.—Thorax.—Lungs not fully dilated with air; lower lobes sunk in water; weight of lungs, 1½ ounce.

CASE LXXII.—Female child, 10 days old. Premature birth. Weighed 3 pounds, 6 ounces.

Appearances.—Thorax.—Lower portion of left lung not dilated; weight of lungs, 1½ ounce.

CASE LXXIII.—Male child, still-born. Weighed 10 pounds, 10 ounces.

Appearances.—Lungs, like liver; weighed 2 ounces.

Case LXXIV.—Male fœtus, about seventh month of utero-gestation; weight 3 pounds, 8 ounces; its mother suffering from gonorrhœa for last three months.

Appearances.—Thorax.—Lungs, like liver, weighed 1⅝ ounce. About four ounces of fluid in the chest, &c.

Abdomen.—Peritonæum opaque and thickened. Intestines adhering to each other, and to the abdominal parietes. Colon adhering to the spleen, the capsule of which was quite opaque. Spleen large, weighing ⅜ ounce; liver weighed 4½ ounces. About four ounces of fluid in abdomen and chest. Testicles had descended into scrotum.

Case LXXV.—Still-born child about the seventh month of utero-gestation.

Appearances.—Abdomen.—Both ureters enormously dilated; appeared like distended, large intestines, occupying the whole front and lower portion of abdomen. Kidneys atrophied, and destitute of the lobular appearance usual in the fœtal state. Bladder much distended with urine. Opening of ureters into bladder free. No obstruction whatever in the urethra.

A preparation of the parts was made by Mr Gulliver, and is now in the Museum at Chatham.

Report* of Dr Boyd, House-Surgeon of the St Marylebone Infirmary, of the admissions into that establishment for the latter half of the year 1839; and of the mortality in the same institution for the whole of that year.

A report of the first half of the year 1839 has already been furnished to the auditors, and published by them in their half-yearly report. The mortality for that period was not included.

The present is an abstract of a return of the cases admitted into the infirmary for the last half of the same year, prepared by the house-surgeon, Dr Boyd, for the Board of Directors and Guardians; to which is added a copy of the return of the mortality for the whole of the year 1839.

The cases admitted during the last six months have amounted to 1457, and have afforded examples of 150 varieties of disease, which have been nosologically arranged in the table, after a simple and comprehensive method—viz. according to the organ or function principally deranged.

The number of deaths for the year has been 322. They are distributed over 50 of the more serious forms of disease, and their arrangement in the table of mortality has been similar to that in the tables of admission. In both tables life has been divided in fourteen periods, so as to show at what particular ages certain diseases are most frequent.

---

* Laid before Directors of Infirmary, 14th February 1840, and printed with general Parochial Report.

I.—Abstract of the Table of Admissions, classifying the diseases under eight heads, and distinguishing the sexes and the number under each disease.

		Males.	Fem.	Total.
1.	Diseases of Digest. organs, viz. stomach, intestines, &c.	39	65	104
2.	„ Respiratory organs—lungs and appendages,	80	86	166
3.	„ Circulatory organs—heart and blood-vess.	28	30	58
4.	„ Genito-urinary—organs of generation, kidneys, bladder, &c.	16	38	54
5.	„ Locomotive organs, and cellular tissue—bones, muscles, &c.	99	91	190
6.	„ Nervous system—brain, nerves, &c.	56	80	136
7.	„ Eye, Ear, and Skin,	257	312	571
8.	Diseases affecting system generally, viz. fever, &c. with which are included some other ill defined conditions, which cannot be arranged under any of the foregoing heads,	59	119	178

II.—Table of the diseases classified under eight heads (as above,) showing their numbers in the several months.

		July.	Aug.	Sept.	Oct.	Nov.	Dec.
1.	Disease of Digestive organs,	15	12	18	11	22	26
2.	„ Respiratory ditto,	26	28	18	12	36	46
3.	„ Circulatory ditto,	11	13	3	13	9	9
4.	„ Genito-urinary ditto,	7	6	9	8	8	16
5.	„ Locomotive & cell. tiss.	27	37	26	29	33	38
6.	„ Nervous system,	25	22	19	22	29	19
7.	„ Ear, eye, and skin,	97	120	101	62	90	101
8.	Conditions affecting system generally,	36	29	29	31	30	23

III.—Table showing the distribution of the cases according to age. It also is an abstract of the Table of Admissions.

Under 1 year	50	From 1 to 2 years,	24	From 2 to 3 years,	44
From 3 to 5 years	126	„ 5 to 10 „	263	„ 10 to 20 „	237
„ 20 to 30 „	131	„ 30 to 40 „	137	„ 40 to 50 „	131
„ 50 to 60 „	127	„ 60 to 70 „	97	„ 70 to 80 „	67
„ 80 to 90 „	18	„ 90 and upwards	5		

IV.—The following Table for the last half of the year 1839 is deduced from the Table of the Mortality for that year, which is appended to this report; and presents 146 fatal cases, classified under eight heads, as in the table of admissions, giving the total number of each class, distinguishing the sexes, and the numbers for the several months.

		July.	Aug.	Sept.	Oct.	Nov.	Dec.	Males.	Fem.	Tot.
1.	Digestive,	8	3	5	1	3	9	12	17	29
2.	Respiratory,	16	9	13	4	10	11	33	30	63
3.	Circulatory,	0	2	0	1	3	1	5	2	7
4.	Genito-urinary,	0	0	1	3	0	1	1	4	5
5.	Locomo. & cel. tis.	2	1	0	1	1	0	3	2	5
6.	Nervous,	2	4	4	2	1	7	8	12	20
7.	Skin,	4	1	0	0	0	1	3	3	6
8.	Affecting system *generally*,	2	1	2	2	1	3	3	8	11

V.—The distribution of the mortality for the six months, according to age, was as follows:

Under 1 year.	10	From 1 to 2 years,	4	From 2 to 3 years,	1
From 3 to 5 years,	11	„ 5 to 10 „	3	„ 10 to 20 „	3
„ 20 to 30 „	14	„ 30 to 40 „	21	„ 40 to 50 „	18
„ 50 to 60 „	18	„ 60 to 70 „	19	„ 70 to 80 „	17
„ 80 to 90 „	4	„ 90 & upwards,	3		

VI.—Comparative view of the Admissions and Deaths for the six months ending December 1839, arranged as in the preceding Tables:

	July.		Aug.		Sept.		Oct.		Nov.		Dec.		Total	Total
	A.	D.	A.	D.	A.	D.	A.	D.	A.	D.	A.	D.	Adm.	Died.
Digestive,	15	8	12	3	18	5	11	1	22	3	26	9	104	29
Respiratory,	26	16	28	9	18	13	12	4	36	10	46	11	166	63
Circulatory,	11	0	13	2	3	0	13	1	9	3	9	1	58	7
Genito-urinary,	7	0	6	0	9	1	4	3	8	0	16	1	54	5
Locomotive, &c.	27	2	37	1	26	0	29	1	33	1	38	0	190	5
Nervous system,	25	2	22	4	19	4	22	2	29	1	19	7	136	20
Skin, &c.	97	4	120	1	101	0	62	0	90	0	101	1	571	6
Affect. syst. gen.	36	2	29	1	29	2	31	2	30	1	23	3	178	11

More than half the deaths in the second class (the diseases of the respiratory organs) were from pulmonary consumption, which disease is ordinarily excluded from the general hospitals; hence a considerable heavier mortality in such parochial infirmaries as St Marylebone, that admits all diseases (except small-pox,) curable and incurable.

Under the seventh head or class, it may be observed, that an epidemic of measles broke out among the children in the Workhouse in the month of July, and that several cases of the same disease occurred in December;—all these cases were admitted into the Infirmary, and some of them terminated fatally from inflammation of the lungs.

VII.—Comparative view of the distribution of Admissions and Deaths, according to age, for the six months ending December 1839:

	Admis.	Deaths.		Admis.	Deaths.
Under 1 year,	50	10	From 30 to 40 years,	137	21
From 1 to 2 years,	24	4	„ 40 to 50 „	131	18
„ 2 to 3 „	44	1	„ 50 to 60 „	127	18
„ 3 to 5 „	126	11	„ 60 to 70 „	97	19
„ 5 to 10 „	268	3	„ 70 to 80 „	67	17
„ 10 to 20 „	237	3	„ 80 to 90 „	18	4
„ 20 to 30 „	121	14	„ 90 & upwards,	5	3

According to the preceding table, the mortality in the early periods of life is greatest. Under one year, viz.—20 per cent. on the admissions;—over five years and under twenty, it is less than at any other period, being about 1¼ per cent.;—between the ages of twenty and sixty, the deaths vary from 11 to 14 per cent.—after sixty and upwards, the deaths amount to 25 per cent. on the admissions.

The oldest death recorded in this report was that of a female ninety-nine years of age.

VIII.—A comparative view of the medical and surgical cases in the St Marylebone Infirmary, for each quarter of the year 1839; showing the number of Admissions, Discharges, and Deaths, the average number of patients of each class, and the average stay of both.

	Admitted.		Discharg.		Died.		No. of days under treat.		Aver. No. in Infirm.		A ver. stay in Infirm.	
	Phy.	Sur.	Phy.	Sur.	Ph.	Su	Phy.	Surg.	Phy	Sur.	Phy.	Surg.
Qr. 31 Mar.	362	280	287	215	79	12	9111	9473	118	157	25½ ds.	38¾ ds
,, 30 June,	356	258	399	271	73	12	11051	15194	109	152	27¼ ds.	43 ds.
,, 30 Sep.	425	318	339	279	70	9	9072	11022	121	159	20¾ ds.	32½ ds.
,, 31 Dec.	415	299	359	327	60	7	8645	8572	103	86	24 ds.	26 ds.

No allusion has been made in this report, either to the numerous sick poor, who come under medical treatment on the Workhouse side of the establishment; or to the still greater number who attend as out-patients, at the Infirmary in the mornings, or who are visited at their own houses. This class of out-door sick, alone, amounts to about six thousand annually.

In the preceding tables are embodied all the information contained in the old registers. A fuller method of registration has been adopted for the year 1840, which, if followed up, will admit of several additional particulars being added, of importance in tables bearing on the subject of vital statistics.

Art. III.—*On some Vicarious Actions of the Human Body, in Health and in Disease.* By J. R. W. Vose, M. D., Senior Physician to the North Dispensary at Liverpool, and formerly President of the Royal Medical Society of Edinburgh.

In a large number of medical writings, isolated observations in relation to the title of the present communication are to be found; but it does not appear that the subject has ever been systematically brought forward by any author. This circumstance, coupled with the great interest which attaches to an investigation so intimately connected with many questions in philosophical medicine, would seem to justify my selecting it for this occasion.

As I have not been able to find, in the works to which I have access, any precise definition of the term Vicarious, as employed by medical writers, I have ventured to suggest the following, to mark the limits within which I wish to confine myself in its use, and to distinguish it with sufficient accuracy from other operations of the economy with which it appears to have been often confounded.

In the subjoined observations, the term vicarious will be used to designate the establishment of a physiological process, or morbid action, dissimilar from another physiological process or morbid action for which it is substituted, and differing, by this dissimilarity, from metastasis or translation, wherein the subsidence of disease in a part is followed by the reappearance of a similar disease in another part.

The study of vicarious phenomena is difficult and perplexing; but so important is a familiarity with them to the scientific practitioner, that he must not be discouraged from diligently observing and arranging the facts which they disclose.

Inquiries of this description are, with much truth, said to be at present in their infancy; but it is not unreasonable to hope, that when patient and industrious observations shall have furnished a sufficiency of materials to warrant generalizations, much that is now obscure and inexplicable in their history will be found under the guidance of simple and harmonious laws.

The importance, in the meantime, of even attempting to assemble and classify observations, now widely scattered, with the view of presenting them in a synthetic form, will be readily acknowledged. Although, in the present paper, it is my intention, on practical grounds, to dwell chiefly upon those vicarious actions which present themselves to our notice when studying the diseased states of the human body, it must not be lost sight of that there is no period, from the evolution of the fœtus to advanced age, during which we have not opportunities of occasionally observing their operation as physiological laws of the animal economy.

It would be tedious to dwell upon this branch of the subject here; but a brief enumeration of some leading facts which it includes, may succinctly illustrate the wider range which is conceded by nature to actions of this description.

To commence with the fœtus. The great size and abundant supply of blood to the liver, the exaggerated developement of the supra-renal capsules during uterine life, point to the probable performance, by these parts, of functions specific in character; for although we cannot indicate these, the ingenious hypothesis of Sir Astley Cooper, that the fluid secreted by the thymus gland serves the same purpose in the animal economy which chyle, a product of digestion does, from the commencement of independent existence, places it within the verge of possibility, that the large dimensions of the organs above-mentioned are also connected with the performance of acts vicariously discharged after birth.

Again, as the body approaches old age, we find the nutrient vessels of various textures and organs deviating from those processes hitherto habitually performed by them, which, usurping, as it were, such as belong to other parts, they make them vicarious of their own. Hence, the bony matters which, in advanced life, are deposited between the coats of the arteries, in place of the yielding elastic tissues proper to those structures, and hence also, among the aged, many of the cartilaginous with other degenerations of the valves and substance of the heart, together with the ossific change undergone by the cartilages themselves. Further, the peculiar bodies termed phlebolites, which are occasionally found in the interior of the veins, have been proved, by chemical analysis, to consist principally of phosphate and carbonate of lime. The presence of these in the vascular system, therefore, demonstrates one of two things, viz. that the salts peculiar to the bones may, under certain circumstances, be formed at the lining membrane of the vessels of soft parts, instead of its natural secretions, or, rejecting their vicarious origin, that they may be generated by a direct combination of their elements in the blood itself. Other interesting facts, analogous in character, might here be adduced. Thus, cholesterine, a substance peculiar to the bile, has been found substituted in place of their proper textures, in many regions distant from the hepatic system, as, in the membranes of the brain and between the tunics of the eye; and, if we direct our attention to the results of mechanical violence inflicted on different parts of the body, we not unfrequently obtain curious information connected with this subject. Our museums show that divided muscles very commonly unite through the intervention of tendon, vicariously formed by their own vessels for the proper tissue, and that, when cartilage is about to undergo a process of exfoliation, its vessels cease to elaborate cartilaginous particles, and become invested with the power of depositing ossific matter in its place.

According to Magendie, after the removal of the kidneys, the secretion of bile is extraordinarily increased; so that the stomach and intestines are found to contain bile in large quantities. This vicarious action is not a little remarkable. The elimination of urea, the most highly azotized of the animal principles, is abruptly suspended; and the immediate result is an increased secretion of bile whose characteristic ingredient is cholesterine, into the composition of which nitrogen does not enter.

It would be easy to amplify this short physiological sketch into a paper of great length; but it will suffice to indicate a necessity which is peculiarly marked in the study of vicarious actions, for bringing a knowledge of the laws of the animal economy to bear upon the diagnosis and management of diseases.

In proceeding to consider, in the *second* place, the pathological part of the subject, I have to regret that my materials are not, as yet, sufficiently extensive to enable me to arrange them according to the anatomical method of textures or organs; but, as some definite plan is obviously requisite, it may be well to select one with a view to practical utility. And here the disposal of the subject under the three following heads naturally suggests itself.

The first will comprise those vicarious actions whose leading characteristic is the relief which they afford, under certain circumstances, to a part or the whole of the system.

The second will include some of those which are unproductive of relief. And, under the third head, will be found illustrations of such as are more pernicious than the diseases for which they are substituted.

I. Under the first head, or that which exhibits the beneficial operation of vicarious actions, the extended class of passive hæmorrhages occurring spontaneously, as a consequence of suppressed menstruation, holds a prominent place. With the distressing train of morbid sensations and perverted functions springing from the disordered states of this important process all are familiar. And that many among them, the obstinate and painful affections of the head, for instance, depend upon local congestions of blood, appears probable, not only from a reference to their cause, but from the fact that we seldom succeed in imparting to these cases even temporary relief, if we neglect the occasional abstraction of an amount of blood equivalent to that which was wont to be discharged from the uterus, and whose retention has induced a redundancy of fluids in the vascular system. Were further confirmation of the truth of this opinion necessary, it would be at once supplied by watching the operations of nature herself, who, if unassisted by art, pours out their contents from the capillaries in different regions of the body, in her efforts to restore the balance of circulation. It would be rather a labour of abstract curiosity than of practical value, to follow out the unaccountable caprices shown by nature, in the different methods which she adopts to disencumber the system of the excess of blood which oppresses it in those cases.

In the majority of instances, it is from some of the principal outlets of the body, that we have occasion to notice the vicarious discharge; giving rise to hæmatemesis, hæmoptysis, hæmaturia, melæna. But there are not wanting observations to show that it may occur at the surface of the body, as in the menstrual ulcer, also from the mammæ, the umbilicus, and even the *tunica adnata*, and the auditory meatus. In a preponderating number of examples, these hæmorrhages appear to be of a passive character, taking place as it were by exudation from the parts which furnish it, being unattended by any perceptible rupture or other lesion of blood-vessels, or by any inordinate action in the heart and arteries.

Many cases of periodical escape of blood from the stomach and mucous lining of the air-passages, in connection with suppressed menstruation, have presented themselves to me; and numerous others are upon record, wherein, during long seasons, and in not a few, for many years, the discharge has taken place habitually, without leading to dangerous results. But that these cases do not always pursue so favourable a course, I have recently had the following opportunity of witnessing.

CASE.—A middle-aged lady had, for a considerable time, been suffering more or less from that painful state of constitutional disturbance which so commonly accompanies the cessation of the catamenia. On one occasion, after experiencing cough with considerable uneasiness in the chest for a few days, her attention was excited by observing a slight admixture of blood with the expectoration. Shortly after this she was visited professionally, and, as the hæmoptysis continued, the thorax was carefully examined; but it was found to have the natural resonance upon percussion, while the respiration, which was uniformly heard, was unaccompanied by any morbid sound. At the same time, there was nothing unusual in the action of the heart, while the pulse was soft and of moderate frequency. The opinion which was formed from these circumstances was, that the hemorrhage probably consisted in a passive secretion of blood at the bronchial membrane, and that it was vicarious of the menses.

The remedial means which appeared to be indicated were at once adopted. The sputa, nevertheless, rapidly acquired the characters of pure blood; and, although never in such abundance as to excite alarm from that circumstance, no measures had any effect in diminishing their quantity, or modifying their appearance. During a space of fourteen days, the patient's state was remarkably uniform. There were occasional periods of languor and depression; but, for the most part, she retained her natural cheerfulness, while the general strength seemed unimpaired. At length a marked prostration suddenly took place; the breathing became hurried and difficult; the countenance proportionally anxious; her efforts at expectoration were less and less successful; and she expired, exhausted, at the close of the third week from the invasion of the attack. To the last, there were no stethoscopic signs of disease within the chest. For upwards of two weeks, the quantity of blood discharged did not vary from day to day to the amount of a drachm; and the mode of her death was by asphyxia, through inability to maintain the exertions requisite to disengage the blood, as it was secreted in the small branches of the air-tubes.

On a careful inspection of the thoracic organs, the pulmonary congestion was found to be great. The bronchial tubes were opened to their minute subdivisions. The mucous coat was, throughout, of a deep-red colour, and somewhat thickened. They contained in abundance a viscid secretion, uniformly stained by blood, and precisely analogous to the matter which had been expectorated during life. On pressing any portion of the lungs, this fluid appeared in abundance at the divided orifices of the air-tubes.

I confess myself to be very much at a loss for a satisfactory *rationale* of this case; but it appears to be one of sufficient interest and novelty to deserve much reflection. It does not seem probable that the fatal event was referable to the hemorrhage alone. This was very limited in quantity; for, although extended through a period of three weeks, there is reason to believe that the patient did not in all lose thirty ounces of blood from the lungs. I have known an individual, labouring under menstrual obstruction, and much below the one whose case we are considering in natural constitutional powers, discharge pounds of blood by vomiting, in a very short space of time, and without the supervention of any approach to exhaustion; and in a lady of remarkably feeble health, whom I have for a long time had an opportunity of observing, considerable quantities of blood are from time to time expectorated, which, from the symptoms, proceed obviously from the lungs, and yet these fits of hæmoptysis are unaccompanied by any distinct disturbance in the system. Were I to hazard a conjecture as to the cause of the obstinacy and fatal termination of the case which has been related, it would be founded upon one of the appearances observed on dissection,—the presence of a glutinous blood-like liquid even in the minute bronchial tubes. The facility with which, from their narrow dimensions, occlusion of this order of the air-passages takes place, and the access of the atmosphere is prevented, by even slight degrees of morbid secretion, can be readily conceived. To remove this mechanical impediment, an increase of respiratory exertion becomes necessary; and if this prove unavailing, progressive extinction of the vital powers ensues, under the preternatural efforts which require to be permanently sustained.

The marked relief which is afforded to congested and other morbid conditions of the brain and liver, by occasional hemorrhages from the nose and rectum, occurring frequently towards the middle periods of life, among persons of sedentary and luxurious habits, need not detain us.

The salutary warning which these accidents impart to the patient, and the clear indications with which they supply the physician, are often followed by the happiest results. The skin, possessing, as it does, a considerable resemblance in its anatomical structure to the mucous membranes, and forming, moreover, an uninterrupted surface with this important class of textures, has, as might be supposed, a very intimate relation with several of the internal parts of the body.

Many valuable facts, illustrating the phenomena of vicarious actions, are to be observed in connection with the functions and pathological states of the cutaneous system. Few of them are more obvious and instructive, perhaps, than those which reveal the close relation subsisting between the skin and the urinary organs. In health, we find that during the warm season, when the transpiration at the surface is copious, the secretion from the kidneys is sparing; while in the winter, when large quantities of fluid are discharged from those organs, the perspiration is found to be scanty.

The same is observed in some diseases, as in diabetes, when the skin is, for the most part, peculiarly harsh and dry, while an inordinate amount of urine is separated from the kidneys. A fact in therapeutics, familiar to practical men, supplies us with further evidence on this head. It has been long observed, that if, during the exhibition of diaphoretic medicines, the surface be kept cool, these remedies very commonly perform the part of diuretics, and an increased flow of urine ensues.

A gentleman with whom I am acquainted informs me, that, during a long term of years, his skin has failed to perspire from those exertions, which in most persons induce an evident moisture upon the surface; for the same period he has been a great sufferer from irritability of the urinary organs, consequent upon the deposition of red sand. When, however, an increase of activity in the cutaneous function is excited by means of warm-bathing, and other means calculated to produce diaphoresis, he is at once sensible of a marked improvement in the condition of the bladder and kidneys. An author, distinguished for his accurate and philosophical researches in medicine, the late Sir Gilbert Blane, whose connection with the public service afforded him satisfactory means of observation on a large scale, has remarked that calculous disorders are most prevalent among persons who are the subject of skin diseases. And Sir Benjamin Brodie, in his work upon the urinary organs, mentions an individual habitually accustomed to indulge in Champagne and other French wines, and who did so with impunity, while he daily induced profuse perspiration, by taking long and rapid walks; but when, from the urgency of his engagements, he was compelled for a season to suspend this exercise, he at once began to suffer from a copious deposition of lithic acid. The kidneys and the skin, from the great abundance of the matters excreted by them, amounting, according to the most approved experiments, in the former case to three or four pints, and in the latter to thirty ounces, in the twenty-four hours, are the most energetic glands of waste with which the system is provided. And some of the foregoing illustrations exhibit the importance which is attached by nature to maintaining the balance of excretion from the economy, by showing the care with which she presides over the vicarious powers inherent in these organs, and developes them whenever the dynamic energies of either are preternaturally enfeebled or increased.

Again, the prevalence of calculous disorders among those who are the subject of long standing skin-diseases, obstructing transudation, and the sandy deposits observed in the urine of individuals in whom the cutaneous excretion is defective from idiosyncrasy or inactivity, appears to admit of a satisfactory explanation by reference to the composition of the last named fluid. Chemical analysis shows that saline and acid ingredients are constituent parts of perspiration; when, therefore, those matters, which are destined to be eliminated, are, from any cause, retained within the system, the lithic acid, whose affinities are weak, is separated from its combinations, and precipitated in a solid form.

In confirmation of the same views, it is important to advert to the relief which is afforded upon the principle of vicarious action, in averting disease from other viscera equally important with the urinary organs, or in mitigating it when established. The late Dr Cheyne, to whom practical medicine is indebted for many researches of inestimable value, has left us some clinical observations distinctly illustrating the success which attends the maintenance, by artificial means, of increased activity in the functions of the skin, when certain internal structures are menaced with disorganizing changes. The remedy upon which he principally relied for exciting transudation at the surface was James's powder; and the morbid tendencies counteracted by the fulfilment of this object, were those whose chief danger consists in local congestions of blood. Thus, where the brain was endangered by a developement towards the middle or advanced periods of life, by what may be denominated the apoplectic diathesis, or when, at an early age, determinations have taken place to that organ, threatening to end by effusion, this antimonial preparation has proved signally efficacious in warding off the impending mischief. In these cases, as well as in others of more general plethora, but in which likewise the head appeared to be the part principally affected, Dr Cheyne had occasion to observe, that although the medicine, for the most part, caused a steady increase of sensible perspiration, yet in many instances, and when it proved equally efficacious, this did not take place, and its appreciable action was limited to a removal of the distressing heat and dryness at the surface, which was generally found to accompany these irregularities of the circulation. These few remarks will serve to demonstrate the assistance which is to be gained, in the management of an important class of diseases, by simply exciting that function which is the skin's peculiar province.

It is not only through its physiological activity, however, that the cutaneous system possesses the power of benefiting the condition of other parts. Many of the morbid changes, to which it is liable, prove unequivocally serviceable in the same way. All must have observed the frequency with which the cerebral disturbance and violent excitement usually accompanying the inflammatory fever by which the exanthemata are ushered in, undergo a marked remission on the appearance of the eruptive stage of those diseases. During many critical periods of infancy and childhood, convulsive and other disorders of a dangerous and intractable character have not only undergone signal relief, but have often been entirely removed by the developement of strophulous, porriginous, and other eruptions. As important structures, in many instances, enjoy an immunity from changes to which they have an inherent, or acquired predisposition, while the surface is the seat of inflammatory processes, it is sufficiently obvious that any attempt to cure abruptly the external disease, and thus to abolish the vicarious action, must, even in cautious hands, be a measure fraught with peril. This is a fact very generally acknowledged; and yet the precautions to which an acquaintance with it should lead, are too commonly neglected in actual practice. A very direct illustration of this came under my notice some time ago. A middle-aged gentleman, of sedentary habits, and who, during many years, had suffered habitually from headachs, became the subject of a chronic cutaneous affection, attended with discharge from the inflamed surfaces. After bearing this for some time, his patience became exhausted, and, in his anxiety to be freed from the complaint, he followed a recommendation inconsiderately given, and repaired to Harrogate. Under the use of the waters at that place, the skin disease was completely removed; but shortly afterwards he suffered an attack of apoplexy, and continued paralytic for the short remainder of his life.

A further exemplification of the mysterious law existing between parts whereby they vicariously relieve themselves from morbid actions, is furnished in many cases of common gonorrhœal inflammation. It is in accordance with the observation of most practitioners, that when the discharge in gonorrhœa is abruptly suspended by treatment or otherwise, it often happens that the testis is attacked with painful enlargement. As soon, however, as the urethral irritation is restored, the affection of the testicle quickly subsides. Another illustration of this department of the subject is not unfrequently noticed in connection with some forms of dropsical effusion; for example, when there is the utmost distress from embarrassed respiration, as in disease of the heart complicated with hydrothorax, the occurrence of anasarcous swelling of the legs is often speedily followed by a marked remission of dyspnœa. If, however, the œdema of the extremities disappear without the simultaneous occurrence of a copious flow of urine, the patient at once relapses into his former state. This singular sequence of actions, in which a formidable disease of the chest is relieved by a dropsical enlargement of parts remote from that region, and continues to be so even after its subsidence, provided this be followed by an increase of activity in the renal functions, is a subject of high practical interest, and well deserves our consideration. I cannot conclude this division of the paper without mentioning a curious pathological fact, which was observed in a case of granular degeneration of the kidneys, that came under my notice several years since. It bears directly upon the phenomena now under consideration. A patient, during the course of this disease, became afflicted with distressing nausea and vomiting. The various remedies which are usually found more or less effectual in mitigating these symptoms were exhibited to him, but they failed completely in affording any relief. At last, with the view of augmenting the discharge from the kidneys, which had become scanty, diuretic medicines were prescribed, and as soon as under their use the urine was increased in quantity, the nausea and vomiting subsided. On several occasions, when the patient experienced a relapse of these symptoms, the employment of diuretics was followed by the same result.

And here it appears desirable to terminate the first branch of the subject in which we have been considering those vicarious actions, whose chief interest is of a practical character, and arises from the relief which their developement affords to parts or the whole of the system when labouring under disease. It would be a task of no great difficulty to multiply extensively examples of the same phenomena; but this were greatly to exceed the limits necessarily assigned to communications of this description.

II. In proceeding, in the next place, to the enumeration of some facts which arrange themselves under the second head, or that which illustrates nature in her unsuccessful efforts to relieve, by the establishment of vicarious processes, the injurious consequences of certain morbid conditions, this subject may be thought to retain an abstract interest only; yet it will probably be found equally deserving of reflection by all who hold the opinion, that it is only by a careful study of physiological and pathological truths, that we can entertain any reasonable hope of advancing the resources of practical medicine.

Some forms of dropsy, such as those which are consequent upon certain physical impressions upon the surface of the body, or diseased actions in the tegumentary vessels, causing a suspension in the functions of the skin, appear to be referable to an attempt at conferring upon internal parts the province of that texture, whereby the system may be disencumbered of the superabundance of fluid, by which it would otherwise be oppressed. Thus in acute or inflammatory anasarca, arising from exposure to cold, or the incautious use of cold drinks when the body is much heated and exhausted by muscular exertion, transpiration from the cutaneous pores becoming suddenly arrested, infiltration of the cellular membrane is speedily substituted.

At the decline of the exanthemata, particularly of scarlatina and measles, a dropsical state often declares itself. The occasion of this does not appear to be very difficult of solution. The perspiration during the course of the eruptive fevers being suspended, in consequence of the capillaries of the skin becoming the seat of an inflammatory erethism, effusion takes place vicariously into the serous and cellular membranes, by which compensation is attempted to be made to the system generally for the interrupted cutaneous exhalation. In these cases, energetic and wonderful as are the efforts of nature to maintain their relative balance among the varied functions of the economy, the result is a disease often most obstinate to remove, and not unfrequently fatal to life. In extending our observations to the phenomena which present themselves during certain disordered states of glandular structures, we often witness a developement of vicarious operation, more complex in their nature than those which we have hitherto been studying, but which are wholly abortive as relates to any advantage which accrues to the system by their establishment. For example, in the *ischuria renalis*, or paralysis of the kidneys, a rare and formidable disease, in which these organs cease to secrete, three circumstances are very constantly noticed amid the intractable train of symptoms which arise. These are, the exudation of sweats having a urinous odour; the appearance in the blood of that peculiar azotised principle, urea, which in health is separated from the kidneys alone; and the accumulation in the ventricles of the brain of a liquid having the sensible qualities of urine.

There is a form of icterus closely resembling the disease last-mentioned in its essential character, since the complete emptiness of the biliary ducts after death, proves it to have been dependent upon non-secretion by the liver, which in the surface exhibits the usual yellowness of jaundice, while cholesterine, the substance proper to the bile, may be detected in the blood by analysis. In neither of these cases is it possible to ascribe the singular circumstances now related to any modifications of absorption, inasmuch as, in either case, the liver and the kidneys have altogether ceased to elaborate their respective secretions. We are therefore forced to the conclusion, that in both the diseases under consideration, the complicated functions of these glands are delegated to the skin and to the blood.

A valuable paper, written by the late Dr Cheyne, entitled a Case of Melæna, contains some observations upon the alternate excess of morbid action in the mucous and serous membranes. The case applies so closely to the present division of this communication, that a condensed view of it may be here offered without apology.

A young man was admitted into the Meath Hospital, on account of a copious discharge of blood, which he had experienced from the stomach and intestines. The hemorrhage was very completely arrested by the repeated exhibition of doses of the oil of turpentine. In two or three days after the suspension of the bleeding, a sense of fluctuation became manifest in the peritoneal cavity. The bowels having now become torpid, Dr Cheyne was induced to prescribe some purgative medicine. This caused several evacuations, and in the evening the discharges of blood were renewed in great quantity. In twenty-four hours from this occurrence, the tension of the abdomen from effusion was less, and upon the following day this tension was scarcely perceptible. The case terminated fatally; and it is remarked, in the account of the dissection, that, " before laying open the cavity of the abdomen, those who were present could not fail to notice the great subsidence of the swelling; and when the cavity was opened, there were found in it not more than two or three pints of fluid." In reviewing this very singular case, although we cannot fail to be impressed with the novelty of the circumstances by which it was characterized, its rationale scarcely admits of misapprehension. A congested state of the abdominal blood-vessels declares itself by an outpouring of their contents from the minuter arteries distributed to the lining membrane of the alimentary canal. Under the employment of certain remedial agents, this is abruptly arrested, and the immediate consequence is an accumulation of fluid in the peritoneal cavity, which, in its turn, subsides with great rapidity, upon the renewal of the hemorrhage by which it was preceded. Here, then, we find nature establishing a process, dissimilar in its seat and product from the original disease of which it is vicarious; and which is instituted with the view of maintaining her efforts to relieve the excited state of the abdominal circulation.

III.—I now proceed to mention some vicarious actions holding their place under the third and last head, which is illustrative of these phenomena, when more pernicious than the diseases for which they are substituted. My supply of materials available for this purpose is very limited.

The best characterized example with which I am acquainted of a vicarious action of this last description, presented itself in a case which came under the care of the late Dr Edward Percival. The patient, a young female, having become convalescent from fever, relapsed with symptoms of unusual debility, and her skin became covered with a red papular eruption. As she had been affected with itch before her admission into the hospital, there were many pustules on the surface, which assumed the worst form of *scabies purulenta*. In a few days after the appearance of the papulæ, she became bloated in a surprising measure; her belly, limbs, and face being so much swelled that her features could not be recognized. On examining the eruption of the skin, its florid hue was found to have nearly disappeared; the smaller pimples seemed to have vanished, and the scabious pustules were dry and shrunk. General dropsy soon declared itself, and the patient expired in about a fortnight after its first appearance. In the account of the dissection, it is stated that, upon laying open the abdomen, the intestines exhibited an extraordinary appearance, being studded with livid spots of various dimensions, from the size of a pea to that of a shilling. The accompanying inflammation of the bowels, moreover, was not merely erythematous or confined to the diaphanous membrane, but penetrated the cellular structure, as was seen evidently in the parts occupied by livid patches; preserving, in this particular, a curious analogy with the morbid condition of the skin, where superficial papulæ were interspersed with ulcerating pustules.

In the foregoing case, the destructive nature of the vicarious processes which ensued upon the sudden subsidence of the cutaneous eruption is well exemplified; and were any further proof of the connection between them and the antecedent disease necessary, it would be furnished by the very remarkable character of the morbid changes which presented themselves after death.

Every physician's experience will have afforded him opportunities of observing facts bearing immediately upon this curious branch of pathology. Most practitioners must have witnessed the occasional developement of anasarca upon the subsidence of typhus fever, and of obstinate ascites at the conclusion of dysentery. Neither can the invasion of fatal convulsions in infancy, or the establishment of incurable mental disorders in more advanced life, upon the spontaneous or artificial suppression of cutaneous eruptions, have escaped notice.

Among the writings of Dr Ferriar, we find it stated, that, upon the removal of chorea, epilepsy is sometimes substituted. The same respectable authority mentions the case of an individual in whom epileptic fits took place upon the cure of itch. The epilepsy resisted all the usual means, and was only removed upon the reproduction of scabies.

It is now full time to conclude this imperfect outline of a subject, which all will be ready to admit is one of deep practical and philosophical interest. To its extent, it is not possible to assign any definite limits, inasmuch as it is constantly obtruding itself upon the attention of those who may devote themselves to the study of the mysterious and infinitely varied laws which preside over the movements of the human body, as well in health as in disease. No one can be more sensible than myself of the great difficulty which attends every attempt to group and to arrange in a lucid manner, the intricate, and often, apparently, contradictory phenomena, which present themselves to us from day to day, in the exercise of the medical profession. Of the success which has attended my pursuit of that object, on the present occasion, I am no judge; but it may, perhaps, be permitted me to add, that it has been my wish, in the preceding pages, to isolate the facts from all reasoning upon them, that their intrinsic value might not be compromised by any fallacy in those inferences which they appeared to justify.

ART. IV.—*Report on the effects of the removal of the 65th, 36th, 69th, 37th, and 8th Regiments, from the West Indies to Nova Scotia, in the years 1838 and 1839. Transmitted to the Director-General of the Army Medical Department.* By J. ELLIOTT, Member of the Royal College of Surgeons, London, Staff-Surgeon, and Principal Medical Officer to H. M. Forces in Nova Scotia, &c. &c.

THE 65th Regiment, consisting of 14 officers and 476 men, arrived at Halifax, Nova Scotia, on the 2d of January 1838, in H. M. S. Cornwallis, from Barbadoes, after a passage of twenty-six days. The corps had served eight years in the West Indies, but had not suffered more from disease than regiments generally do on that station. At the time of embarkation, the men were comparatively healthier, and on landing at Halifax, the acute diseases were but few. Many, however, laboured under chronic affections of the thoracic and abdominal viscera, consequent upon long residence in a tropical climate and habits of intemperance.

On approaching Nova Scotia, and also after landing at Halifax, the men, women, and children on board suffered much from the severity of the cold. The embarkation of the regiment was so immediate, that time was not afforded to supply the men with woollen clothing adapted to a North American climate in the winter season; but blue cloth trowsers were issued to them on board by the purser of the Cornwallis. The women and children were supplied with articles of warm clothing, on their arrival, by the charitable donations of the inhabitants of Halifax.

Although the sick on landing were not numerous, consisting principally of chronic diseases of the lungs and bowels, yet, from the effects of exposure to intense cold and intoxication, the admissions into hospital became very frequent during the first month, and many of the diseases were severe and dangerous. Care was taken to have frequent medical inspections of the men, and directions were given to send even the slightest cases to hospital, by which means the health of the soldiers rapidly improved, and the number of sick diminished.

In the course of three months, the regiment had made considerable advancement in health and efficiency; and although the men were not, perhaps, in a state fit for field-service, yet they were sufficiently recovered for the performance of garrison duty. During this period, more of the men from the West Indies were admitted into hospital than of those in the garrison, who had never served in a tropical climate; and many of the diseases of the former class were not only more severe in character, but more disposed to a fatal termination. Of the draft of 134 men, mostly young soldiers, that joined from England early in the spring, the admissions for a time may have been equal in number to those of men from the West Indies; but this was depending on many of these being young and delicate subjects, easily affected on their first arrival from Europe by the climate, during the season in which cold and wet weather prevailed.

The left wing of the 65th Regiment proceeded from Halifax to New Brunswick, in the second week of January 1838, by sea, when two companies, on landing, went to Fredericton on the ice; and one company was left at St John for garrison duty. The right wing, head-quarters, followed early in April. On the removal of these divisions from Nova Scotia, the sick of the corps were left in hospital at Halifax.

Early in June following, the whole of the regiment was concentrated at Fredericton, and the men had then become greatly improved in health and appearance. The barracks not being sufficiently capacious to contain the whole of the regiment, 100 men were encamped in the barrack enclosure, and were relieved weekly by the same number from the barracks. Notwithstanding the weather on some days in June was very wet, yet those under canvas enjoyed equally good health with the rest; a few catarrhal cases only occasionally occurring among them.

Many of the men that were in bad health on landing in North America, and who had suffered much from disease in the West Indies, greatly improved in health soon after their arrival, under careful hospital treatment and salutary exercise.

The diseases to which the men from the West Indies were most liable in Nova Scotia and New Brunswick, during the cold season, more especially, were those of an inflammatory character, affecting mostly the lungs and bowels. In cases of chronic visceral disease of long standing, inflammation of the affected organs was often superinduced; and in such circumstances, the cases became somewhat intractable, and difficult to be relieved by the best remedial measures. Relapses of intermittent fever, —a disease scarcely known to originate in Nova Scotia, occurred in some of the men who had suffered from that disease in the West Indies, and these were generally of the tertian form. Common fevers, under which head many cases admitted with mere ephemeral vascular excitement are included, often the result of intoxication, were frequent. Ophthalmia, principally of the catarrhal character, prevailed among those who had suffered from the disease in the West Indies, apparently excited by the vicissitudes of the weather, and aggravated by excessive spirit-drinking. Many of the cases that occurred in those who had severely suffered from the complaint in the West Indies were obstinate, and in some few vision became more or less defective.

In the third quarter of the year, from 1st July to 30th September, the weather was fine and favourable to health; and the regiment had become so healthy and efficient as to excite, by the appearance of the men on parade, a degree of surprise in the minds of some of the oldest officers on the station.

The 65th Regiment was orderd on service to Canada early in November 1838, and embarked at Chediac, New Brunswick, for Quebec, in the highest state of efficiency.

Return of the number of sick of the 65th Regiment during the first nine months after their arrival in North America from the West Indies.

From 2d January to 31st March 1838.

Admitted,	177	Of the 5 deaths, two were from chronic dysentery, one from chronic catarrh, one from diarrhœa, and one from common fever.
Discharged,	161	
Died, .	. 5	
Remained,	11	

From 1st April to 30th June 1838.

Admitted,	224	Of the 9 deaths, two were from pulmonary consumption, one from chronic catarrh, one from dropsy, two from fever, one from disease of the liver, one from colic, and one from vomiting of blood.
Discharged,	203	
Died, .	9	
Remained,	23	

From 1st July to 30th September 1838.

Admitted,	153	Of the 2 deaths, one was from dysentery, and one from diarrhœa.
Discharged,	159	
Died, .	. 2	
Remained,	15	

The 36th Regiment, consisting of 13 officers and 430 men, embarked at Barbadoes, in H. M. S. Hercules, on the 10th of November 1838, and arrived at Halifax, Nova Scotia, on the 8th of December following, after a service of eight years in the West Indies. The corps had suffered much from remittent fever when stationed at Antigua, and subsequently from dysentery at St Lucia, Demerara, and Barbadoes.

At the time of embarkation, the regiment was comparatively healthy, with the exception of some cases of chronic dysentery, which promised but little chance of ultimate recovery.

During the early part of the voyage to Nova Scotia, the corps did not suffer from any particular prevailing disease or privation; but on approaching the colder latitudes of North America, the men, women, and children felt the severity of the cold weather; the chronic dysenteric affections became aggravated, and of these two died on board, after entering the harbour of Halifax. Major Crosse of the 36th Regiment died of fever on the third day after embarking at Barbadoes, and before the Hercules sailed.

The men were provided with warm clothing at the time of embarkation.

On the landing of the regiment at Halifax, the men appeared much affected by long service in the West Indies. They looked sallow; and the constitutions of many were evidently much broken, from climatorial causes, and, perhaps, habitual intemperance. No particular disease prevailed at the time, excepting some chronic pulmonary and dysenteric affections, originally contracted in the West Indies, and which became more or less aggravated by the almost sudden transition from extreme heat to intense cold.

After landing, the admissions into hospital became numerous, particularly during the first fortnight, and exceeded, in a great proportion, those of the other corps then in garrison, and those last from England, who had never served in the West Indies. The most frequent diseases were inflammatory affections of the lungs and bowels, consequent upon exposure to cold, intemperance, and subsequently the early movements of the regiment to the frontier of New Brunswick, when the quarters, from necessity, were both indifferent and comfortless.

In consequence of the invasion of disputed territories by the Americans, the 36th Regiment, composed entirely of men from the West Indies, was sent, in two divisions, from Halifax to St John's, New Brunswick, by sea, to replace the 11th Regiment, that had gone on to Canada by the land-route. The first division proceeded on the 15th of December, a week after landing in Nova Scotia, and the second in the course of the week following, in H. M. steamer Medea. All the actual sick were left in hospital at Halifax, by which arrangement much inconvenience and bodily suffering were prevented on the landing of the corps in New Brunswick, at such an inclement season of the year.

From St John, the regiment proceeded to Fredericton, and the greater part went thence to Woodstock, and other stations on the frontier of New Brunswick, along the course of the river St John. The men in those expeditions suffered from continued exposure to cold and indifferent quarters; but the cases of disease were by no means numerous, probably from the active mental and bodily exertion attendant upon these movements. Catarrhal affections were the most frequent; but they admitted of early relief. Several were frost-bitten,—an accident which mostly occurred to men while in a state of intoxication. The only serious case of this kind happened to a deserter, who had lost himself in the woods.

In May a draft of 134 men arrived from England. In June, the whole of the regiment was concentrated at Fredericton, having been relieved at Woodstock and other advanced posts on the frontier, by the 69th Regiment from the West Indies, via Halifax and St John. The corps had now so much improved in health that, at the half yearly inspection, the major-general commanding was much pleased with its appearance and efficiency. Another draft of sixty-two men arrived from England in September.

From the arrival of these drafts in New Brunswick, the proportion of admissions into hospital of men from the West Indies, compared with those direct from Europe, did not greatly differ. The ratio of mortality of men from the West Indies was four to one of those last arrived from England. It may be observed, that the latter arrived at a favourable time of the year, in May and September, both healthy months. The greater number of deaths occurred previous to the arrival of the drafts from the depôt.

The men whose health had been impaired in the West Indies, and were not labouring under serious organic disease, improved in every respect, and apparently became equally healthy with those who had never served in a tropical climate.

The diseases to which the men from the West Indies were most prone after their arrival in North America may be stated to have been catarrhal and bowel affections, in which the inflammatory symptoms became easily excited by incautious exposure to the severity of the weather in the winter months, and perhaps habitual intemperance. Abscesses and ulcers were also frequent; many of the former extensive, and for a time intractable. The admissions of men from the West Indies have been far more numerous than of seasoned soldiers of other corps on the station, who had never served within the tropics.

Several cases of erysipelas, of a very severe type, occurred in the summer and autumnal months of 1839, affecting mostly those whose constitutions had been impaired by a West India climate and intemperance, and of which some terminated fatally. The disease was prevalent at this time among the inhabitants of Nova Scotia and New Brunswick, apparently of an epidemic character, and not depending upon the change from the West Indies to North America. Ophthalmia prevailed in the corps in the West Indies, and after its arrival in New Brunswick. Many cases came under treatment at Fredericton, but the proportion of admissions into hospital from this disease of men from the West Indies has not been greater than of men who arrived direct from Europe.

Return of the number of sick of the 36th Regiment in the first seven months after its arrival in North America.

From 8th to 31st December 1838.

Admitted,	86	Of the 6 deaths, one from disease of the lungs, and five from chronic dysentery.
Discharged,	57	
Died,	6	
Remained,	23	

From 1st January to 31st March 1839.

Admitted,	125	Of the 7 deaths, three from disease of the lungs, three from chronic dysentery, and one from delirium tremens.
Discharged,	103	
Died,	7	
Remained,	15	

From 1st April to 30th June 1839.

Admitted,	179	Of the 8 deaths, five from disease of the lungs, one from apoplexy, and two from delirium tremens.
Discharged,	152	
Died,	8	
Remained,	34	

The 69th Regiment, consisting of 13 officers, and 446 men, embarked at Barbadoes in the Crocodile frigate, Numa, and Elizabeth Transports, in January 1839, and arrived at Halifax, Nova Scotia, in the course of February following. The first division in the Crocodile was 25 days, the second in the Numa, 44 days, and the third in the Elizabeth, 29 days, on the voyage.

The men of the 69th Regiment were comparatively healthy during the passage from the West Indies to Nova Scotia, and no deaths occurred. They were provided with warm clothing, and did not suffer from any privation on board.

At the time of the arrival of the regiment in Nova Scotia, the weather was exceedingly cold, and the frost more severe than usual; the thermometer at times falling many degrees below zero. The harbour of Halifax became rapidly frozen over; an occurrence that but rarely happens. The men of the corps suffered much from cold during the latter part of the voyage, while detained in harbour at Halifax, and subsequently on the passage from thence to New Brunswick.

The several divisions of the regiment, as they arrived at Halifax, were ordered round to St John, New Brunswick, by sea, in consequence of the invasion of the disputed territory by the Americans of the state of Maine. A strong draft of 185 fine healthy young men arrived from England in February in the Inconstant frigate, and after landing, proceeded with the first division in the Crocodile. The sick of the regiment, on leaving Nova Scotia for New Brunswick, were few in number, and were left in hospital at Halifax. Four of these died, and those that recovered subsequently joined the corps in the latter province.

The Elizabeth transport, with head-quarters and two companies, was unfortunately wrecked off Cape Sable, on her passage from Halifax to St John, New Brunswick. The troops were landed at Shelburne by a timber vessel that was passing at the time. No lives were lost; but the officers, men, women, and children, suffered greatly from exposure to extreme cold and wet during the occurrence. This detachment was afterwards taken to St John by the Crocodile frigate.

The divisions of the 69th, as they landed at St John, with the exception of one company left for garrison duty there, were advanced to Woodstock, 150 miles distant, and 60 above Fredericton, on sleighs and sleds upon the ice, from whence detachments were sent to Tobique and the grand Falls on the River St John.

No particular disease prevailed in the corps on its arrival in North America. The men from the West Indies suffered more from cold than those recently from England, and those of other corps on the station who had not served in a tropical climate.

The regiment, although greatly exposed to cold during its movements on the frontier of New Brunswick, was during the time much more healthy than could have been anticipated. There were but few cases of sickness reported, and these were in a less proportion to those of other troops in quiet garrisons; which circumstance may, perhaps, be attributed to the precaution taken in leaving all the sick in the hospitals of Halifax, St John, and Fredericton, *en passant*, and to the mental stimulus and bodily exercise attendant on these military movements. The difficulty of obtaining spirituous liquors, unless in moderate quantities, may also have afforded further means of preserving the health of the soldiers at this period.

The proportion of men from the West Indies admitted into hospital to those of the corps who had never served in a tropical climate, the latter mostly young soldiers, was, during the first twelve months from the arrival of the regiment in North America, nearly equal. The ratio of mortality in the same period, in these two classes, was about one in 22 of men from the West Indies, and one in 93 of the drafts from Cork.

The men whose health had been impaired by long service in the West Indies, excepting those in whom organic disease had made serious inroads on the constitution, improved greatly in health and appearance after the arrival of the corps in New Brunswick, and in most of them recovery was effected.

The diseases to which the men from the West Indies were most prone after their arrival in North America, have been relapses of ague, common fevers, and catarrhal affections. Cases of rheumatism and bowel complaints have also occurred.

Ophthalmia prevailed in the corps in the West Indies, also previously in Ireland in 1829, and after its arrival in New Brunswick; but not at the latter station in greater proportion among the men from the West Indies, than among the young soldiers that came direct from Ireland.

The health and efficiency of the 69th Regiment may be considered to have been established in six months from its arrival in North America. That the corps should have experienced such an immunity from disease during its movements on the frontier of New Brunswick, immediately after landing in the cold and boisterous month of March, has been a matter of surprise to every one, and can only be attributed to the circumstances stated in another part of this report.

Return of the number of sick of the 69th Regiment in the first eight months after arriving in North America.

From 3d February to 31st March 1839.

Admitted,	148	The one death was from disease of the lungs.
Discharged,	121	
Died,	1	
Remained,	26	

From 1st April to 30th June 1839.

Admitted,	184	Of the 11 deaths, two from fever, six from disease of the lungs, one from chronic dysentery, one from inflammation of the stomach, and one from delirium tremens.
Discharged,	180	
Died,	11	
Remained,	19	

From 1st July to 30th September 1839.

Admitted,	217	Of the 3 deaths, one from fever, one from disease of the lungs, and one from chronic dysentery.
Discharged,	210	
Died,	3	
Remained,	23	

The 37th Regiment, consisting of 15 officers, and 424 men, embarked at Jamaica in H. M. S. Hercules on the 21st of March 1839, after a service of six and a-half years in the West Indies, and two years previous service in Bermuda, and arrived at Halifax, Nova Scotia, in April following, after a passage of 27 days.

The mortality at Bermuda, exclusive of accidental deaths, amounted to 32, and at Jamacia to 223, during the before-mentioned periods; but although the regiment frequently suffered from visitations of sickness, yet it may be considered to have been healthy in comparison with other corps on the Jamaica station.

At the time of embarkation, the regiment was particularly healthy, and no sick were left behind. The latter consisted chiefly of chronic cases, and they were enabled to embark with the corps for Nova Scotia.

On approaching the coast of North America in the month of April, the weather was extremely cold and wet, and which was much felt by the men on board. Diarrhœa and dysenteries were the principal complaints treated on the passage, and three of the latter had a fatal termination. The dysenteric cases were much aggravated by the cold and wet weather towards the latter part of the voyage. Each soldier was provided with two flannel shirts, *and they* wore trowsers of blue serge.

When the regiment disembarked at Halifax, the weather was still cold and wet, and which soon caused a considerable number of admissions into hospital; and for some time after the arrival of the corps, the men from the West Indies suffered more from the effects of cold and disease than those recently from England, and those of the other regiment then in the garrison (23d,) who had never served in a tropical climate.

A draft of 98 men had arrived at Halifax from England in the Inconstant frigate in February, previous to the arrival of the regiment from Jamaica, and another of 123 men arrived in the month of August following. These drafts consisted mostly of very young soldiers.

The diseases from which the men of the 37th Regiment mostly suffered after their arrival in Nova Scotia, were relapses of intermittent fever, pulmonic and bowel affections, extensive abscesses, many seated in the axilla, and erysipelas of a severe kind, and of which latter, one died in June and another in August. This disease, however, was not the consequence of removing from the West Indies to North America. It was prevalent at the time among the natives of the province, and was apparently of an epidemic character.

Although, after the arrival of the regiment in Nova Scotia, the admissions into hospital were comparatively numerous, yet, with the exception of the first three months, the mortality was small, the deaths having been five in the first quarter, and four only in the following six months, and these were mostly cases of pulmonary consumption.

The proportion of men from the West Indies admitted into hospital in the first year after the arrival of the corps in Nova Scotia, was 792½ per 1000, and of those who had never served in a tropical climate, 879½ per 1000. It may be stated that the numerous admissions of the latter class resulted from the drafts from home having consisted of very young men, of whom many had tender constitutions, and also became easily affected by the vicissitudes of the weather and intemperance. The ratio of mortality in these two classes was 1 in 60 of those who had served in the West Indies, and 1 in 112 of those who had never been in a tropical climate.

The health and strength of the men greatly improved after their arrival in North America, including even those who had suffered from bad health in the West Indies.

The particular diseases to which the men from the West Indies were most prone in the first year after their arrival in Nova Scotia were, 1. relapses of intermittent fever, the number of which amounted to 50, whereas, not one case occurred in those direct from Europe; 2. catarrhal affections, in the ratio of one in eleven, of men who had served in Jamaica, and one in eight of those who had not been in that island; 3. bowel complaints as one in eleven who were in the West Indies, and one in thirteen of those who were never in a tropical climate.

Ophthalmia did not prevail to any extent in the 37th Regiment when in the West Indies; nor has it been prevalent since the arrival of the corps in Nova Scotia; and it may be remarked that the disease has not occurred in men on the latter station who had suffered from the complaint while resident in Jamaica.

The head-quarters of the 37th Regiment have remained at Halifax. Two months after the corps arrived in Nova Scotia, one company was detached to Prince Edward's Island, and another to Cape Breton, on the usual annual tour of duty. In the course of six months from its arrival in North America, the regiment regained such a state of health and efficiency, as to render it fit for any kind of military service.

Return of the number of sick of the 37th Regiment in the first nine months after their arrival in Nova Scotia.

From 1st April to 30th June 1839.

Admitted, .	199	Of the 5 deaths, two from disease of the lungs, one from chronic dysentery, one from erysipelas, and one from disease of the liver.
Discharged, .	164	
Died, .	5	
Remained, .	30	

From 1st June to 30th September 1839.

Admitted, .	192	Of the two deaths, one from disease of the lungs, and one from erysipelas.
Discharged, .	195	
Died, .	2	
Remained, .	25	

From 1st October to 31st December 1839.

Admitted, .	173	The two deaths were from disease of the lungs.
Discharged, .	168	
Died, .	2	
Remained, .	28	

The 8th or King's Regiment, consisting of 25 officers and 547 men, after serving five years and ten months in Jamaica, embarked at Montego Bay in that island, on the 17th of April 1839, for North America, and arrived at Halifax, Nova Scotia, on the 5th of May following. The regiment had suffered much from tropical disease, and the mortality had been great. At the time of embarkation, the regiment was comparatively healthy. Seven sick were left in Jamaica, not so much from being unfit for removal, as from want of accommodation in the vessels of war that conveyed the corps to Nova Scotia.

The regiment was healthy during the voyage, and did not suffer from any particular privation; but when within three days' sail of Halifax, the cold experienced was comparatively great; the thermometer having fallen to 40° of Fahr. No deaths occurred during the passage from the West Indies.

The men were not provided with warm clothing until they arrived at Halifax, when they received flannel shirts and drawers. They wore blue serge trowsers on the voyage.

After landing in Nova Scotia, the admissions into hospital were for a time more numerous than those of other men then in the garrison, who had not served in the West Indies. 183 were admitted from the 10th of May to the 30th of June. The prevailing complaints were catarrh and diarrhœa. Of the 183 admitted, 77 were of the former, and many of the latter disease. Several relapses of ague also occurred. The diseases admitted were mostly of a mild character, and none terminated fatally. Some chronic diseases of the thoracic and abdominal viscera existed among the older soldiers, induced by climate and intemperance, of whom the majority were sent to England for invaliding and recovery in September following.

The regiment may be considered to have been healthy since its arrival in Nova Scotia; the health of the men rapidly advanced during the summer and autumnal months; and no deaths occurred until November, when a man died from carditis, and another from erysipelas; a disease then prevailing in Nova Scotia. These two deaths could not be referred to the effects of change of climate.

The whole of the men of the 8th Regiment came from the West Indies. A draft of 149 men arrived in Jamaica from Europe a month previous to the departure of the corps for North America, and who could scarcely be considered as having served in a tropical climate.

In regard to the ratio of mortality in these two classes of men, of the two fatal cases, one in each took place.

Of the 22 men sent to England in September, whose health had much suffered from long service in the West Indies, it is not known how far recovery has been effected among them. Several of these were old worn-out soldiers, recommended for discharge.

The men, whose health had been impaired by a West India climate, and who did not labour under intractable chronic visceral disease, greatly improved in health, strength, and appearance, after the arrival of the corps in Nova Scotia.

The diseases to which the men were most prone after their arrival were catarrhal and bowel-affections. These, however, were for the most part of a mild character, and yielded to prompt medical treatment. Relapses of intermittent fever were frequent in those who had suffered from the disease in Jamaica. The paroxysms were speedily removed by the use of quinine and a few days confinement in hospital.

The proportion of admissions into hospital of the men of the draft from England that joined a month before the corps left Jamaica, to those of the older soldiers who had long served in a tropical climate, has been one of the former to three of the latter class.

Ophthalmia prevailed in the corps while in the West Indies, but never to any great extent. None have been admitted into hospital with this disease since the arrival of the regiment in North America.

At the termination of the quarter ending 30th September, five months from the arrival of the corps, the men had so improved in health and efficiency, as to be considered fit for any kind of active service.

The rapid improvement of the health of the 8th Regiment, and two deaths only having occurred in the first eight months from its arrival in Nova Scotia, both from accidental causes, may be principally attributed to the corps having arrived at a mild and favourable season of the year.

Return of the number of sick of the 8th Regiment from the 6th of May to the 31st December 1839.

From 6th May to 30th June 1839.

Admitted, .	183
Discharged, .	159
Died, .	0
Remained, .	24

From 1st July to 30th September 1839.

Admitted, .	242
Discharged, .	249
Died, .	0
Remained, .	30

From 1st October to 31st December 1839.

Admitted,	204	Of the 2 deaths, one from carditis, and one from erysipelas.
Discharged,	189	
Died,	2	
Remained,	30	

From the foregoing observations upon the effects produced by a North American climate on soldiers that have arrived from the West Indies in the winter season, and the advantages experienced by those who arrived in the spring months, it will appear evident that it would be desirable in future that troops from the West Indies should reach Nova Scotia or Canada at a time of the year favourable to their health, and comfortable to their feelings.

As in the early part of the spring the weather is generally cold, wet, and variable, I am of opinion that the troops should arrive in North America towards the latter end of May, or early in June. Regiments from the Mediterranean, intended to relieve those on the West India station that are destined for Nova Scotia or Canada, might leave Gibraltar about the beginning of April, when those relieved would be enabled to leave the West Indies for North America early in May. These troops will then have five successive fine months to recruit their health and strength, and will become gradually inured to the cold of the first winter.

It may be remarked, that the most common diseases of soldiers in North America, during the cold winter season, are of an inflammatory character, involving the lungs more especially; and that it is to the frequent repetition of these inflammatory attacks, induced by exposure to the weather and acts of intemperance, that the majority of cases for invaliding and of mortality are to be attributed. Soldiers arriving from a tropical climate in the winter season are more liable to these attacks than others on the station, who have never served in the West Indies. Relapses of intermittent fever in those who have been subject to the disease in the West Indies also frequently occur from the extreme cold; and the various bowel affections, to which men had been previously subject in a tropical climate, become aggravated, and occasionally have a fatal termination. Rheumatic complaints are also, at times, excited by incautious exposure to cold and wet weather.

In addition to the effects produced by extreme cold, the disposition to the drinking of ardent spirits becomes increased, with the idea of inducing temporary corporeal warmth, which cannot fail to afford a further source of disease and suffering; and in those who may be labouring at the time under chronic derangement of the lungs or bowels, high inflammatory symptoms are sometimes superinduced, and from a repetition of attacks, the diseases, in some instances, ultimately become intractable, and the subjects either die in hospital, or are rendered unfit for further military service.

The immunity from serious disease and mortality experienced by the 8th or King's Regiment, during the first six months after their arrival in Nova Scotia in May, affords a convincing proof of the propriety of removing troops from the West Indies to North America at a season favourable to the re-establishment of their health and efficiency. An objection, perhaps, may be made to the sending of troops from the Mediterranean to the West Indies in the spring season of the year; but, from a long residence in the West Indian islands, I am inclined to think, as the rains commence in May and the increase of temperature is so trifling, that the health of the soldiers who have been serving some years in Gibraltar or Malta will suffer much less by removing to the West Indies at the period recommended, than has been generally supposed; besides, the voyages would be effected during fine weather,—a matter of no little importance to the health and comfort of the troops embarked.

Halifax, Nova Scotia,
June 1840.

ART. V. — *Brief Notes on the Lake-Fever of Canada.* By THOMAS STRATTON, M. D., Navy Medical Department.

ON the shores of Lakes Ontario and Erie, Lake-Fever is the popular name of intermittents and remittents. Farther back in the country they are generally called ague.

As the tide of immigration advances westward, these, the prevailing diseases of Canada, retire before it. Kingston, situated at the eastern extremity of Lake Ontario, is in 1840 very much healthier than it was in 1830; and Dunville, 200 miles, and Amhertsburg 430 miles farther west, where now there are several cases of ague in every house in the course of the year, will probably in other ten or twenty years be as healthy as Kingston is now. Besides there being less fever at Kingston than at the other two places mentioned, its type is intermittent, while at Dunville and Amhertsburg it is often remittent.

Cleghorn, Fordyce, and some other writers, consider ague infectious; but, in numerous opportunities I have had of inquiring into the accuracy of this observation, I have never noticed the disease to be propagated by infection or contagion.

The effect of sulphate of quinine, when not equal to stop the return of the paroxysm, is sometimes to make its accession an hour or so later, as in the following case.

Peter Heaton, aged 30, 1839, September 14th, had rigors at 1 P. M.; 15th, at 2 P. M.; 16th, at 3 P. M.; 17th, at 4½ P. M.; 18th, at 5½ P. M.; 19th, no paroxysm; 21st, cured.

In some cases the paroxysms occur at the same hour during the continuance of the disease, as will be seen in the following case, which also illustrates the premonitory yawning.

William Glover, aged 21, 1839—

Sept. 12,	occasional yawning from 9 A. M.	to 12 A. M.;	rigors at 12 A. M.
13,	do.	do.	do.
14,	do.	do.	do.
15,	do.	do.	do.
16,	do.	do.	do.
17,	do.	do.	do.
18,	do.	do.	do. 12½ A. M.
19,	none		none
21,	cured		

The explanation of the yawning may be, that, while the cold stage is about to come on, there is congestion of blood in the internal parts, and among others in the brain, lessening a little its sensibility, and making it less sensible to the demand of the system for the continuance of the function of respiration. The premonitory yawning occurs in perhaps one-tenth of the cases.

Another symptom which often comes on an hour or two before the accession of the cold stage is coughing, which likely is from congestion of blood in the lungs.

The vomiting in the cold stage of ague is often very severe; and I lately knew of a case where hernia was produced by it. When this symptom is very troublesome, I have often checked it by opium.

When the chilliness of the cold stage continues long, the patient derives great relief and comfort by taking a dose of from 10 to 15 grains of camphor.

In the hot stage, the symptom most distressing to the patient is severe headach. This I have often relieved or removed by having him removed into a strong current of air; at other times giving from 20 to 30 minims of laudanum, (as advised by Lind,) has afforded speedy relief. The opium has had this effect in every instance in which it has been used, in general sleep not being induced by it, merely removal of the headach.

In many instances the use of emetics is unnecessary, from the great vomiting which naturally occurs in the cold stage; and in other cases, I have not observed any particular good result from them;—many patients to whom emetics were not given recovering as soon as others who took several in the course of the disease.

A cure was found to be produced in the shortest time by administering calomel and compound extract of colocynth; and when three or four evacuations had been procured, giving a large dose of the sulphate of quinine, without much regard to the intermissions. Some recommend the quinine to be given in the intermissions only; others advise the whole quantity to be given in one dose immediately before the expected paroxysm; but as it is likely that the medicine requires an hour or two to produce any effect, its exhibition, in most cases, during the paroyxsm, is not contraindicated, and by waiting for the intermission its effect upon the first subsequent paroxysm is rendered null.

Venesection in the cold stage was found by the late Dr Mackintosh to shorten the cold stage, and break the habit of the disease. In four cases only in which I tried it, its effects were to alleviate and shorten the hot stage, the course of the disease not being stopped, and not appearing to be made shorter. During the last two and a-half years I have been in Canada, most of the people I have seen affected with ague have been spare-bodied and weakly; and if venesection in them were to fail in cutting short the disease, as it did in the four instances above-mentioned, it would most likely make the cure more tedious by adding to the patient's weakness. Many medical men in the country with whom I have conversed on the subject consider venesection in the cold stage objectionable in most cases, and in the remainder not to have the effect experienced by Dr Mackintosh.

In the popular treatment of ague, much use is made of emetics (generally of mustard); and the medicines with quack names, of which the people make use, are often, I believe, solutions of the sulphate of quinine.

When the patient is recovering there is often an eruption on the lips.

At Kingston, in ten months, July 1838–April 1839, in 150 men, there were seven cases of intermittent. I am informed that about ten or twelve years ago every third or fourth person in the neighbourhood was the subject of ague.

At Dunville and Amhertsburg, in ten months, May 1839–February 1840, of 70 men there were 51 attacked by marsh-fever.

Kingston, north latitude 44° 18′; west longitude 75° 41′; 234 feet above the level of the sea, has a population of 6000. The country round is hilly, mostly cultivated, and generally free from marshes. The Cataraqui bridge connects the town with a low peninsula, many hollow parts of which are below the level of the lake, and filled with water or dry, according as the water of the lake is high or low. The part of the peninsula alluded to is bounded by Navy Bay and Ereen Bay on two sides, and on other two by the "Common," and the hill on which Fort Henry is built. As at present vessels for the Rideau Canal pass through a part of the Cataraqui bridge to the interruption of passengers, there has been some talk of making a canal through the neck of the peninsula; and if this ever be done, the excavations from the canal will fill nearly all the hollows alluded to, and remove, in a great degree, the causes of what little lake-fever Kingston has still to complain. There is one and sometimes there are two regiments at this place, which is also a naval station.

Dunville, north latitude 42° 33′; west longitude 79°; 364 feet above the level of the sea, is on the left bank of the Grand River, five miles from where it flows into the eastern extremity of Lake Erie. The river is about fifty yards wide; the surrounding country thickly wooded, flat, and marshy; the banks of the river in many places scarcely a foot higher than the surface of the water. At Dunville there is a dam across the river for canal purposes, which has made the banks above the dam to be much overflowed, and also the banks below it, from the circumstance that certain winds making the water at the east end of the lake rise considerably, and flow up the river,—this reflex current, which before the existence of the dam extended eighteen or twenty miles up, being now stopped, five miles up, overflows the banks to a considerable extent. The residents in the neighbourhood are very subject to marsh fever, every family having several ill in the course of the season. The unhealthiness of this place will likely continue without any decrease, as it is not probable the dam across the river will be removed. This is a naval station.

Amhertsburg, north latitude 42° 36′; west longitude 82° 56′; 364 feet above the level of the sea; on the left bank of the river Detroit, one mile from where it flows into the western extremity of Lake Erie. The surrounding country is flat, and about half cleared of wood. Close to the town is a creek, which is filled chiefly by the overflowing of the river, and emptied by evaporation. By damming the mouths of this creek, where it is connected with the river and lake, and by draining, this marshiness might be removed; and this will no doubt be done in the course of ten or twenty years, when land shall become more valuable. Ague and remittent fever are very common, most families having one member at a time ill during the summer and autumn. In summer the heat is very oppressive. This is the most westerly British military station in America, and as it is the most southerly part of Canada, some may perhaps feel interested in an account of the temperature for twelve months.

1839	April	range of thermometer,	38°	to 79°
	May,	do.	46	84
	June,	do.	50	85
	July,	do.	61	84
	August,	do.	60	84
	September,	do.	47	78
	October,	do.	42	75
	November,	do.	2	60
	December,	do.	15	43
1840	January,	do.	5	45
	February,	do.	10	64
	March,	do.	23	60

It is not probable during winter there can be a first attack of intermittent, as the marshes are sealed by the frost. Any case that occurs is most likely from exposure to wet in a person predisposed by having had the disease in summer.

I spent the winter of 1839–40 at Amhertsburg, and during it there were very few cases of marsh-fever; but on February 17th, there was a thaw, the ice over the muddy bank of the river floated away, and next morning several persons living in houses along the bank were attacked by ague. There is a regiment stationed here, and it is at times a naval station.

Sandwich is a small town, fourteen miles north from Amhertsburg, and similarly situated to that place as regards creeks, &c. Government has this year (1840) completed the erection of new barracks. There are two companies of a regiment stationed here; and I am informed that often one-half of the men at a time are ill of fever. The inhabitants are very subject to marsh-fever. This is occasionally a naval station.

Windsor is a small village, two miles north from Sandwich, and also on the river Detroit. The bank is here high, and there are no creeks as at the two towns last mentioned. New Barracks have been built here this year (1840), which are at present occupied by militia. There is no ague here, though only two miles from a town where remittent fever is very general, and occasionally fatal. It is probable that the gaseous poison, which is the cause of intermittent fever, is not in a state of chemical union with the common air, but in a state of mixture only. Often in these marshes, early in the morning, I have observed a stratum of air two or three feet thick, resting on the ground, and of a darkish colour, and at the same time have felt a very offensive odour. These I have observed often in the evening also, and then along with them a peculiar sensation of heat. These three phenomena have been noticed by persons walking with me, and who were not, like perhaps myself, looking for them.

Samia and Goderich, on Lake Huron, are very healthy localities. It is observed that militia regiments do not suffer so much from marsh-fever as regiments from Britain. In winter, as regards health, troops may be stationed at Sandwich with as much propriety as at Windsor. An objection to their being stationed in summer at Windsor is, that there is a greater facility for desertion here than at Sandwich; but this advantage is gained by the loss of the use of half the strength who are in hospital during the autumn.

Kingston, Upper Canada, December 1840.

Art. VI.—*Contributions to Pathology.—Rupture of the Liver—Rupture of the Spleen—Empyema,—Epilepsy and Apoplexy.* By Alexander Kilgour, M. D.

I.—*Rupture of the Liver and of the Spleen.*

A considerable number of cases of rupture of the liver and spleen may be found scattered through medical authors; but I do not think there is one where the accident was of the nature of the following case.

Case I. *Rupture of the Liver.*—Christopher Brown, aged 36, a shoemaker, was brought to the hospital on the evening of the 8th September, from the watch-house, with a note to the effect, that it was a case of inflammation of the bowels.

At the visit on the forenoon of the 9th, he was found complaining of severe pain over his bowels. The abdomen was very tense, tympanitic, and the least pressure gave great pain. His countenance was anxious and contracted; his skin not morbidly hot nor cold; his pulse 108, sharp, small, and wiry; his tongue brown and much furred. He had no vomiting, nor had he retched any. His bowels had not been open, he stated, for several days; had slept none during the night; and was constantly moaning and complaining of his bowels. The account given, according to the statement of the nurse, on inquiry from those who brought him in the police truck, was, that he had been picked up in a gutter in a state of complete intoxication on the previous evening, and been taken to the watch-house, and that he had complained of constant pain in the bowels, and inability of moving from the pain.

Sixteen ounces of blood were ordered to be drawn from the arm; and twelve leeches were directed to be applied to the abdomen, and afterwards emollient poultices. A spoonful of castor-oil was ordered to be taken every two hours, till the bowels should be freely moved.

10th. Died this morning at seven o'clock, having been sensible to the last.

Inspection, twelve hours after death. No external appearance of injuries. On the scalpel entering the abdomen bloody fluid gushed out. The fluid was collected, and amounted to ten pounds, (twelve ounces to the pound.) Some coagula were found, but almost the whole was dark fluid blood. On passing the hand into the pelvis, to examine whether the bladder had been ruptured, a body was taken out, which was at once seen to be the gall-bladder, and which was about half-distended. It was now evident that the cause of death was the separation of the gall-bladder from the liver, and the consequent effusion of blood. On turning up the liver it exhibited a rough surface, corresponding to a similarly sized rough surface on the upper part of the anterior aspect of the gall-bladder, being the part where it is united to the liver. Coagula were adherent to the under surface of the liver, and to the transverse arch of the colon, and were also lying between the liver and duodenum. The texture of the liver was infiltrated with blood for about two inches in depth, and a fissure to the size of an inch in length, extended inward from the fossa of the gall-bladder, and was about half-an inch deep.

The gall-bladder was about half-filled with a mixture of bile and blood. The intestines were much inflamed, being in some parts of a deep-red colour, and in others dark blue.

The brain and viscera of the chest were healthy.

It may be here stated, that there were rumours soon afloat, that this man Brown had been struck by the watchman on the night that he was taken to the watch-house. The case was taken up by the proper authorities, and at the trial at the circuit Court of Aberdeen, evidence was laid that Brown had been struck, and that the watchman had jumped upon his belly with his knees. The prosecutor for the crown departed from the plea of murder, and a verdict of culpable homicide was brought in, on which the watchman was sentenced to banishment for life. It came out on the trial that there had been a collusion to conceal the nature of this case at the watch-house, or, at any rate, such irregularities in regard to reporting it in the proper quarter, as to call for a severe reprimand from the judge.

On looking through the remarks that have been made by morbid anatomists on the subject of ruptures of the liver and spleen, there will be found some points that admit of a few comments, and which the above and following cases may serve to illustrate. Dr Baillie says, " the liver is more liable to be ruptured by external violence than any other gland of the body, which probably arises from two causes. The one is, that in thin persons the liver, more especially when large, lies near the surface of the body, and therefore may be readily affected by a strong external pressure ; the other is, that the liver consists of a structure, the parts of which are more easily separated from each other by pressure, than those of almost any other organ of the body. * * * When ruptures have taken place in the liver, they have happened from some strong pressure applied to the upper part of the abdomen, as, for instance, from the wheel of a carriage passing over that part of the body. Little pain has been felt from such an injury ; which is a proof, among many others, of the liver not possessing much sensibility ; and some of the persons to whom this accident has happened have lived for several days."—Morbid Anatomy, Lond. 1833, p. 196.

1. Is the liver more liable to be ruptured by external violence than any other gland of the body ? If we are to be strictly confined to " glands," Baillie is probably right ; but there is little doubt he was thinking of a comparison with the other solid viscera in the abdomen. If so, the recorded cases would appear to give a preponderance of ruptures of the spleen ; and other authorities are opposed to Baillie on this point. Platner says, and Morgagni who quotes him, seems to agree with him, " that rupture or laceration happens more frequently in the spleen than in any other viscus : and the latter authority continues ; " nor is this to be wondered at, if you attend to the soft substance thereof, and the thinness of the coat wherewith it is invested. But if it become softer by the force of

disease, and, by a quantity of slowly circulated blood, is extended below the fortification of the ribs, it is much more easily ruptured; and pours out in less time the greater quantity of blood: and consequently brings on a more speedy death." Translat. by Alexander, Vol. iii. p. 212. But of this organ, Baillie says; " The spleen has sometimes been known to be ruptured in consequence of external pressure upon that side of the body where it is situated. When the spleen is of the ordinary size, an accident of this kind can very rarely take place, because it is well defended by the ribs of the left side; but when the spleen is very large, so that a part of it passes below the margin of the ribs into the cavity of the flank, such an accident may very rarely happen." Morbid Anatomy, p. 216. Baillie would appear not to have known much of the histories of cases of this kind; and his opinions, both in regard to rupture of the liver and of the spleen, seem to have been founded upon their comparative exposure to pressure or external injury. The faithfulness of Morgagni's statement as to the quantity of blood poured out, and the speedy dissolution, is supported by the following case that I saw, and with the notes of which I have been furnished by my colleague, Mr Keith, in one of whose wards he was.

Case II.—*Rupture of the Spleen.* June 19th 1838. James Sinclair, mason, aged 33, was brought into the hospital at one P. M., having been precipitated from a height of twenty feet along with the ruins of an old house, which he was engaged in pulling down. He was partly buried among the bricks and rubbish, and required to be dug out.

He was exceedingly faint; the surface of the body blanched and bloodless; the pulse almost gone at the wrist; he complained exceedingly of thirst; he laboured under great difficulty of breathing, and coughed up mucus mixed with blood; the belly was tense, full, and tender; several of the ribs were fractured. No effort could produce reaction, nor heat the body or limbs in the least. He continued sensible, however, to the last, and expired at half-past four P. M., exactly four hours after the accident befel him.

Twenty-four hours after death the body was examined. Nine ribs were fractured at or near their angles; several had penetrated the substance of the lung on the left side. The abdomen was full to distension with effused blood, which had flowed from the vessels of the spleen, as this organ was found smashed into a perfect pulp, completely broken up and separated into small portions. The liver and other viscera in the abdomen were uninjured.

2. Baillie also thinks that " strong pressure" must be ap-

plied to the upper part of the abdomen, such as the wheel of a carriage; but in fact a blow of almost no magnitude may produce this effect, and it is probable that sudden and violent muscular contraction may produce the same. In the precognition (private judicial examination previous to committal of a prisoner for trial,) a stick with a round knob or head, such as watchmen carry, was exhibited to me, and I was asked if the injury could have been caused by a blow with it; and I had no hesitation in saying that it might; and in this opinion Dr Ogston concurred with me. Heister notices (Inst. Chir. p. i. lib. i. cap. 15. Not. a) a case of a boy who died from laceration of the abdominal viscera from being struck by his schoolmaster, "*non nisi bacillo quodam tenuiori, sed paulo tamen vehementius.*" Pearson gives a case where the liver was almost entirely divided, where the young man fell from the sixth step of a ladder, striking the right hypochondrium and epigastrium on the edge of a pail which he carried in his hand. (Med. Trans. of the College of Physicians, Vol. iii. p. 378, Lond. 1785.) Abercrombie relates a case where a man, sitting carelessly on the edge of a cart, was thrown from it by a sudden jerk upon the road. He got up immediately, and scrambled into the cart, which was still in motion, and he did not appear to the person who was with him to have suffered any injury, but he soon became faint, and in a few minutes was dead. On inspection the liver was found to be ruptured through a great part of the right lobe, and there was extensive hæmorrhage in the cavity of the abdomen." (Pathol. Researches on Stomach, &c. Ed. 1837, p. 355.) But it is not in the liver alone that we meet with rupture from such slight causes. Bohn relates a case where the spleen of a woman was ruptured so that she died in a short hour by a stroke on the left hypochondre from her husband "*baculo mediocris crassitiei;*" and also another where extensive rupture of the same organ was produced by a cudgelling *(ex fustigatione)*, whilst there was not the slightest discoloration *(ne suggillatio quidem)* of the integuments, (De Renunciat. Vulner. p. 383, Lips. 1689.) This circumstance, the absence of any external mark of injury, is carefully noted by the old writers as one of the most remarkable points connected with these cases; and in some seems to be the principal reason for recording them. In Brown there was no discoloration of the integuments, otherwise it would have been entered in the report. Heister records one where the liver was found entirely broken down from external violence, and yet no lesion was observed externally. (Loc. citat.)

3. Baillie states, that "little pain has been felt from such an injury, which is a proof among many others of the liver not

possessing much sensibility." Now, how stands the fact? Brown, according to the report, had very great pain; and on the trial, it came out, that he complained of the most agonizing pain from the moment he got the injury, and considered himself as a dead man. In Mr Pearson's case, the pain is described as being excruciating from the beginning to the end. In a case of hæmorrhage from spontaneous rupture of the liver given by Sir Gilbert Blane, but which the history of the case would seem to connect with slight external injury, the patient was seized with severe pain in the left hypochondrium; and the left lobe of the liver was found on examination after death to be the seat of a rupture. (Trans. of a Soc. for the Improvement of Med. and Surg. Knowledge, Vol. ii. p. 18. Lond. 1800.) Morgagni quotes a case from Lanzoni, where "one who was struck with a fist violently on the liver, and had in his belly an extravasation of blood and a rupture of the vessels in the liver, fell down upon the ground soon after receiving the blow, and expired in a miserable manner." Having neither Morgagni in the original, nor the authority to which he refers, I take Alexander's translation, as intimating that the patient died in much agony.

A case is given in the 18th volume of the Med. and Phys. Journal, of a man who was struck after he had taken a hearty meal, with the muzzle of a carronade, in the right hypochondrium. He fell down on the deck, complained of acute pain, and expired in about twenty minutes. There were no marks of violence externally; the abdomen was filled with blood, and a rupture of the right lobe of the liver extended to the *lobulus Spigelii*, disuniting the gall-bladder partially from its natural situation.

It was proven in the trial of the watchman, that Brown had been eating, and, more particularly, drinking freely before he was struck, which, from what occurred in this case, would seem to bring the gall-bladder more within the influence of external injury.

In the case given by Dr Abercrombie, there is no mention as to whether the patient felt much pain, but the speedy supervention of fainting might have prevented the pain. Andral relates a case of spontaneous rupture, where the patient, not previously exhibiting any symptoms of disease of the liver, on awakening one morning, felt rather ill, and complained of some pain in the abdomen. He expressed a wish to remain in bed, and was left alone, when after some hours he was found dead. (Clinique Medicale by Spillan, p. 899.) I am not wishing to call in question the opinion entertained by Baillie and others, as to the little sensibility of the liver generally, but merely to show that pain is a prominent symptom in rupture of that organ, whether that pain depends upon the injury of the liver itself, or on inflammation excited in the peritoneum and intestines.

4. Dr Baillie states in conclusion, that some of the persons to whom this accident has happened have lived for several days. This quite accords with the existing histories of the several cases. Death is the result either of the loss of blood, the anæmia by effusion into the peritoneal cavity, or of inflammation in the serous membrane of the abdomen. Dr Abercrombie's patient died almost instantly after the injury. Sir Gilbert Blane's case survived only five or six hours, the patient having fallen into a swoon, from which he never recovered. Mr Pearson's lived only ten hours, the pulse not being perceptible from the first, and the extremities becoming colder and colder. Andral's patient also lived only a few hours. The following case, where I assisted at the dissection, is confirmatory of the rapidity with which death takes place, and also so far as to the pain attending such injuries.

Case III.—*Wound of the Liver.*—A man, name unknown, was found in the morning lying dead in a retired part of the Links, with a discharged pistol beside him. He had been seen from a rising ground in the vicinity, a short time before he was found dead, crawling about and writhing his body; but as it was supposed to be some person in a state of intoxication who had slept there, no farther notice was taken of him; others, it was proven, had seen him late in the evening walking near the same place. When the body was examined, a wound was found in the right side over the region of the liver, and which had penetrated the right lobe in an oblique manner from before backwards. There had been no ball in the pistol, but the wadding was found imbedded at the bottom of the wound. The abdomen contained a large quantity of blood partly coagulated.

All these are examples where the individuals died from the immediate loss of blood. Brown lived, however, above three days, and his death was perhaps more attributable to inflammation than to loss of blood, notwithstanding the quantity found in the abdomen. Morgagni quotes from the *Commercium Literarium* an example of a man being violently struck by a horse in the region of the liver; so that the lower ribs were broken, and the gibbous part of that viscus was cleft with a great number of fissures, not very deeply, however, as I suppose, since the belly was not filled with blood but with a great deal of bloody lymph: and the patient did not die before the fourth day. (Transl. Vol. iii. p. 214.) He also refers to a case by Hippocrates, where a boy received a kick from a mule on his belly and liver, and died on the fourth day. (Epid. l. v. n. 39, Not. n. 17, as Morgagni says.) But, as there was no dissection, Vallesius commenting on the case, thinks that the boy died from inflammation of these viscera, and not from rupture or the effusion of blood. Bohn quotes a case from Tulpius, where a wound of the liver, attended with hemorrhage, was followed by death on the fortieth day, and on dissection a collection of pus was found in the liver at the seat of the wound. (De Renun. Vul. p. 368.)

5. Are there any symptoms by which we can discover rupture of the liver or spleen, where there is no perforating wound or external mark of injury? The only symptoms which afford a diagnosis from rupture of any of the hollow viscera appear to be those depending upon loss of blood; paleness of the countenance, fainting, indistinctness of the pulse, and rapid dissolution. There is the same acute pain attending rupture of the liver or spleen with effusion of blood, as rupture of the intestine and effusion of its contents. But in those cases where the individuals live for some time, have we any symptoms by which we may know that the liver is injured? Hennen says, "the usual symptoms which characterize these injuries are, yellowness of the skin and urine, derangement of the stomach and alimentary canal, and cutaneous affections, particularly great and distressing itching."—(Military Surgery, 2d ed. p. 430.)

Bohn also tells us (chiefly from Celsus, however,) that there is anxiety of the præcordium, faintings, pain in the neck and shoulders, vomiting, sometimes bilious, sometimes bloody, bloody stools, heat, and intense thirst. P. 364. In Brown there was neither yellowness of the skin, nor derangement of the stomach, nor bowels, nor vomiting. In those cases where there was an external wound, besides hemorrhage in several instances by the wound itself, there was more or less of a discharge of bile, or of a bilious-coloured glutinous fluid. Thomson and Hennen are at variance as to the existence of jaundice from wounds of the liver. (Report on Military Hospitals, &c. Edin. 1816, p. 101.) There are no symptoms by which we are likely to be able to distinguish rupture of the liver from rupture of the spleen. Some light may probably be thrown on the case by a history of the mode of injury. Strokes and blows on the right hypochondrium are most likely to injure the liver; whilst falls from a great height, or general compression of the body, as where an individual is buried under a fall of earth or part of a ruin, is most likely to be followed by rupture of the spleen. The recorded cases seem to show that rupture of the spleen is more speedily fatal than rupture of the liver, unless where the latter is very extensive; a circumstance which we might almost infer from the structure of the spleen.

There are very few examples collected of rupture of the gall-bladder without injury of the liver itself. There is a well known case in the Med. Chir. Trans., Vol. iv. p. 330, of a stroke from the shaft of a cart on the region of the liver, followed by bilious vomiting; and where, three weeks after the injury, the belly, which was distended with fluid, was tapped on three different occasions, and forty-one ounces of what appeared to be pure bile taken away, the case terminating successfully. But the nature or composition of the fluid was not accurately examined, and it still remains a question whether this was bile or not. There is a case given by Thomson, however, where a collection of bile was found contained in a cavity formed by adventitious membrane, after a wound of the liver itself. Fallopius and Forestus hold wounds of the gall-bladder to be necessarily mortal, but not immediately so; whilst Helmont and others entertain the opinion that they are instantly fatal. Elliotson says, (Lectures by Rogers, p. 875,) "Now and then the gall-bladder has been ruptured. A woman came one day to St Thomas's Hospital, and fell down dead; and, on opening her, the gall-bladder was found to have been suddenly ruptured. Death, I understand, took place instantly."

6. What will be the prognosis in rupture or wound of the liver? On this point authorities differ very much. Celsus places wounds of the porta of the liver as fatal, and wounds of the substance of the liver amongst those *quæ vix ad sanitatem perveniunt*, Lib. v. Cap. 26. Galen, commenting on one of the aphorisms, (6. Aph. 18, and not Aph. 15, as Bohn says,) seems to think, along with Hippocrates, that wounds of the liver are always fatal; at the same time that he was aware, that cases of cure, not only of wounds, but where part of the liver had been cut away, had been given. It would appear that, in the time of Galen, a distinction was made betwixt wounds of the substance and wounds of the vessels of the liver; the latter being considered mortal, the former not so. But Bohn, strongly inclined to Galen's opinion, thinks there is here something like a distinction without a difference, and holds that, at the utmost, a possibility of cure can only exist where the superficies of the organ is injured. It is by no means unusual in *post mortem* examinations to meet with cicatrices, lines, or depressions, in the surface of the liver; and though it is probable that these, in the majority of cases, are the sequelæ of abscesses, they may be in some instances the result of ruptures of the substance of the organ. One of the most satisfactory cases of recovery after injury of the liver is one quoted from Hildanus in the Sepulchretum, p. 16–18. A man received a wound in the region of the liver, and when the surgeon proceeded to examine it, he

found a piece of the liver projecting in the wound, which he seized with his forceps and cut off. Severe symptoms ensued, but he recovered entirely. Three years after he died of continued fever; and, at the necropsy, they found a small portion of the lower part of the lobe wanting, and the wound *cicatrice eleganter obductum.* The experience of modern observers leads to the conclusion, that wounds and ruptures of the liver are not fatal, unless very extensive. " A deep wound of the liver," says Hennen, p. 429, " is as fatal as if the heart itself was engaged. The slighter wounds are recoverable, particularly if the membrane alone is injured." Thomson in his report, p. 98, says, " we saw twelve cases of wounds of the liver, in which considerable progress towards recovery had been made." So that the prognosis is not so entirely against the patient as the old writers supposed.

II. *Empyema.*—Case I.—The following case exhibits in the treatment a circumstance (not usual in these cases, though, I think, very necessary in any case under the same state—the giving free exit to air, as well as to purulent matter.

Alexander Clark, aged 23, a mason, was admitted into St Luke's ward, October 5th 1839, complaining of difficulty of breathing and slight pain in his side, attended with great emaciation, much feeling of weakness, want of appetite, looseness of his bowels, and occasional sweatings; skin hot; tongue clean; pulse 104; urine containing an abundant lateritious sediment. Has some cough, and expectorates, without difficulty, a small portion of a clear frothy fluid. Lies almost invariably on his left side, or inclined to it; and the difficulty of breathing is much increased on his attempting to turn to his right side. Respirations 56 in the minute.

The left side of the chest is distinctly fuller and rounder than the right. The *sulci* betwixt the ribs are quite visible on the right side; but the left side presents a smooth uniform fulness. It does not move with respiration, as is the case with the right. No pulsation of the heart can be seen on the left side, and the hand laid on the precordial region can scarcely feel it; but on the right of the sternum, and below the nipple, its pulsations are visible to the eye, and its stroke sharp and strong to the hand. The integuments, discoloured with blisters which have been applied, are œdematous and pit on pressure. The sound is very dull on percussion over the whole side, and no respiratory murmur can be heard. The left side is fully two inches larger than the other one, the measurement being taken below the nipple. Betwixt the fifth and the seventh ribs, and posterior to their cartilages, there is, to the eye, rather more fulness or elevation of the integuments.

The ribs are widely separated, and there is a feeling to the fingers of fluid seated at some depth.

States that he was in ordinary health until four weeks ago, when he came home in the evening from his work in a state of perspiration, and changed his shirt, putting on a cold one, and went out. He came home, and went to bed about his usual hour, and soon after felt a most acute pain in his left side, which continued of the most intense character until six o'clock in the morning, when he sent for a medical gentleman, who immediately bled him largely, which was followed with great relief. He also got large doses of tartar emetic, and on the eighth day after, he was out of bed. On the fourteenth day after the attack he went out, and on returning home, he was seized with cold shivering, which returned upon him more than once. Since this time has felt his breathing short, and the weakness has increased very much. States also, that, a year ago, he coughed up a little blood; but he was never disabled from working.

This was clearly a case of empyema following pleuritis. There was no communication with the lungs, so far as the physical signs or the character of the sputa went. Although not stated in the report, as drawn up by the clerk, yet the right lung was found to be free from disease. On raising him up and bending the body forward, a projection of the integuments betwixt the sixth and seventh ribs became much more distinct, and the feeling of fluctuation very palpable. This, therefore, made the place of the operation, which, after consultation, was resolved on, not that of choice but of necessity; but the situation was sufficiently dependent; and the only further desirable circumstance would have been to have had the presentation of the fluid rather nearer the spine than the sternum. Accordingly, on the third day after an opening was made by my colleague, Mr Keith, at the projecting part, the integuments, &c. having been divided with a scalpel over the upper edge of the seventh rib, and a canula introduced, through which thirty ounces of thick creamy purulent matter passed in a full stream. The canula was then plugged; but, half an hour afterwards, there not being much relief in the breathing, the canula was withdrawn, and thirty-six ounces more of purulent matter suffered to escape from the wound. It was allowed to run so long as it came in an uninterrupted stream, but when it began to come out in jets it was stopt by laying in a piece of lint, and applying over it slips of adhesive plaster.

October 10th. Passed a quiet night, and slept for several hours; was occasionally troubled with cough; pulse 108. The lint inserted in the wound was taken out, and fifteen ounces of purulent matter escaped; but it was ejected in spirts at each inspiration, and a portion of atmospheric air got entrance; for, on laying the ear to the chest, there is heard a gurgling sound, and at the nipple there is an emphysematous tumour, which rises and falls with respiration. The side is much reduced to the eye.

October 11th. Slept well; pulse 100; lint withdrawn, when twelve ounces of purulent matter, somewhat fetid, ran out. Its discharge was assisted by pressing below the diaphragm on that side. The emphysematous swelling is greater around the nipple.

October 12th. The amount of discharge twelve ounces, fetid, but thick.

October 14th. Eight ounces yesterday and the same to day; but the compress came off yesterday, and much fluid escaped in the bed.

October 16th. Was very comfortable yesterday, and the plug was not removed. To-day, on removing the dressing, there has been a discharge of thin fetid purulent matter, to the extent of between two and three pounds. During the night he had great difficulty of breathing, and frequent cough, with abundant expectoration, which he feels coming from the affected side.

On the 17th, the report informs us, that the wound had burst twice since the previous day, and that above a couple of pounds of fluid of the same fetid thin appearance had escaped. On the 20th, he was so much better, that he insisted on getting up to try on his waistcoat, which buttoned over him; a property it did not possess previous to his entering the hospital. No discharge from the wound, which appears to be healed up. On coughing, the integuments around the nipple are much blown out. The side by measurement is only about a quarter of an inch larger than the other. The wound, opened with a probe, gave exit to four ounces of purulent matter. Appetite good; a piece of sponge tent was put into the wound. On the 21st, respiration is not heard on the application of the stethoscope, either anteriorly or laterally. The heart is advancing to the left side, and may be seen in part pulsating to the left of the sternum. Chest sound svery loud on percussion on the left side.

The case continued in much the same state; but on the 2d November, the report states, " only a discharge to the extent of eight ounces since last report, (28th October,) and the incision to-day appears healed. He has much cough, and has expectorated since last night, three times the fill of the spit-box, (holding about twenty ounces.) Complains much of pain about the nipple." It appeared to me that the external air, which had got admission, was keeping up the irritation, and that, as it naturally rose to the upper part of the cavity, and the tendency of the wound to heal up with probably the formation of false membranes, prevented its escaping in the same way as it entered, it would be advisable to make another opening higher up, where the blowing out of integuments intimated a communication already existing betwixt the pleura and integuments. Pent-up air is injurious, but air applied to an open surface, no matter of what extent, is not nearly so. Accordingly, to allow of a free exit to the air, an opening was made in the centre of the emphysematous swelling, and the lower wound was again opened. Both openings were after this time kept open with sponge tent, and the openings were, by this means, enlarged to the full space betwixt the ribs, so that by placing a *speculum vaginæ* over either of them, and holding a candle to the end of it, the *pleura pulmonalis* could be seen of a vivid red colour, and its distance from the ribs ascertained. Twelve ounces of fetid pus came from the upper opening, and next day twenty-four ounces. On the day following, there was the same quantity. After this, the case went on for some time very well, the pus not being above four ounces daily, often much less. On the 16th November, the report says, " Yesterday about three ounces of pus; after which two ounces of a solution of sulphate of zinc, in the proportion of one grain to the ounce of water, were thrown into the upper opening, and allowed to remain for a few minutes. It gave very little pain, and run out on his turning to his belly." On the 20th, " a solution of sulphate of zinc, in the proportion of three grains to the ounce of water, has been injected. Discharge, as before, but thinner. It is emptied out daily when he sits up or turns over to his belly." On the 14th, it is stated, that " he has daily a discharge betwixt two and three ounces in quantity, sometimes thick, at other times thin, and rather fetid. The space betwixt the ribs and *pleura pulmonalis,* which, previous to the last injection, was three-quarters of an inch wide, is now considerably less."

Hitherto he had had frequent perspirations, especially during the night, and his pulse was quick. After this time, however, the general symptoms improved very fast,—the heart had passed over to its natural position nearly, and respiration was heard by the stethoscope nearly as well in the left as the right lung. The report on the 29th is, " General appearance considerably improved. Is out of bed daily for a short time. On taking the plug out of the wound at the visit, there is a discharge of a fluid, at first like water, and afterwards a little purulent, the whole not amounting to two ounces; and this is the whole collected in the course of twenty-four hours."

December 6th. "Was continuing to improve till yesterday morning, when he had a rigor. The discharge amounts to two ounces." He now complained of much pain in his right side about the infra-mammary region. He had great difficulty of breathing, slept little, and perspired most profusely; pulse 130. Leeches were applied to the right side, followed by mustard poultices; but the symptoms did not yield, and he died on the 8th December.

It was ascertained, that, not satisfied with being out of bed, and walking through the ward,—from being most anxious to get home, and believing himself almost quite well, he had gone out, to try his strength, as he said, on the day previous to that on which he had the rigor.

Sectio Cadaveris, twenty-six hours after death. Body considerably emaciated; chest gave a clear sound on both sides, and not much more so on the left than on the right side. A dark-coloured fluid ran from the upper opening, on turning the body to the side. On raising the sternum, the pericardium was seen nearly in its natural situation, being rather more under the sternum and to the right side than usual, but not to such a degree as to call for particular notice. The upper opening, which was betwixt the fourth and fifth ribs, led into a cavity, the boundaries of which were the diaphragm below, the pleura and lung to the inside, and the walls of the chest to the outside, the root of the lung behind, and above false membrane, which fastened the apex and part of the lobe of the lungs to the ribs. This space held rather more than the open hand betwixt the lung and the wall of the chest. The lower opening betwixt the sixth and seventh ribs was closed by false membranes. There were no tubercles in the lung, which seemed to be nearly of its normal size. On placing it in water, it floated. There was a small piece of about a nut, of a white solid cheesy matter, near its root. Around this, the lung was condensed, and showed red hepatization, but not extensively so. The root of the lung was broken in some parts; but this was believed to be owing to its being torn in separating it from the adjacent adhesions, which were very strong. The part of the lung opposite to the upper opening was covered with a thick layer of a reddish lymph, or rather organized membrane, for the extent of a hand-breadth. The right lung was quite healthy internally, but its lower lobe was covered with large quantities of recently effused lymph,—the result of the pleuritic attack which carried him off.

The *post mortem* examination, it must be observed, was made under adverse circumstances; and the chest was not allowed to be opened in the way that would have been preferred, as that most likely to exhibit the exact state of the parts. There were discharged 346 ounces of purulent matter, besides what run into the bed, and could not be collected.

Notwithstanding that the rule in practice to make the opening, where the feeling of fluctuation or the protrusion of the integuments indicates that ulcerative inflammation would probably in process of time achieve this end, is upon the whole most advisable, yet, if this afterwards prove insufficient for emptying thoroughly the collection of fluid, there can be no reason why the same rule should not also apply there as in other cases of abscess, and a more dependent opening be made. I soon became aware that this opening was too far forward, and that the pus secreted from the walls of the cavity did not get a free escape, as shown by its running out latterly only when he turned himself on his belly. Should a case of the kind occur to me again, I would, though performing the operation of necessity at first, keep in view that of choice afterwards; and I intended, in this case, before allowing him to leave, and after giving him a little time to ascertain if the discharge would dry up otherwise, to have passed a long probe to the lower and posterior part of this cavity, and made an incision upon it.

It is I think almost impossible to prevent the entrance of air even with every caution. The action of the lungs favours the entrance of air in a remarkable degree, and though in many cases, the lung of the affected side is not acting, yet the increased expansion of that of the opposite side pushes over the mediastinum, and to a certain extent there is the same expansion and contraction of the area of the affected as of the sound side. False membranes are formed, or adhesions take place, with extreme rapidity in serous membranes; and we cannot rely upon the passage to our external opening being free. It is of the greatest importance to prevent the confinement of air, and the opening for that purpose in the above case was followed by immediate improvement in the symptoms.

There is another circumstance worth notice in the case, relatively to a valuable communication on the subject of empyema in the 17th number of the Dublin Journal of Medical Science by Dr Greene, and that is the occurrence of a most abundant expectoration on the temporary closing up of the external wound. The report does not state whether the sputa were fetid, mucous, or purulent; but we gather that the symptoms were not improved. There was no fistulous communication by the bronchial tubes with the purulent collection; and if, as Dr Greene *thinks,* there does, in some of these cases, a metastasis take place, or a critical evacuation from the bronchial tubes, there was no relief afforded here. No notice is taken of expectoration after the second opening had been made, so that it had most likely ceased.

Case II.—The following case is against the opinion of Begin (Dict. de Med. et Chirurg art. Empyème), as quoted by Dr Greene, "that of the two methods of evacuation of the matter of empyema, that by thoracic abscess is always more advantageous than the route which the collection sometimes takes by perforation through the bronchial tubes, inasmuch as, in the latter instance, lesions of the pulmonary tissue are often superadded to the primary affection."

Mrs Bate, aged 40, married, was admitted, 10th December, with typhous fever, accompanied with the usual exanthema of a bright florid hue. On the 15th she had symptoms of pneumonia, attended with cough and rusty sputa. On the 31st, erysipelas attacked a swelled gland which she had in the axilla and extended down the arm, and travelled in an erythematic form over the whole back down to the nates and perineum; and afterwards attacked the face and head. On the 6th of January, the erysipelatous redness was nearly gone, she was sleeping well, her skin moist, and pulse at 80, the face being still swelled and eyelids closed. On the 11th a large abscess was forming under the scapula of the right side, and appearing below its inferior angle. A few days afterwards this abscess was opened. On the 4th of February, the abscess still discharging by the opening, she complained of pain in the axilla, and also of some pain in a soft puffy swelling, but without redness, immediately above the right mamma. She had a painful short cough, without expectoration, and a catch in her breathing for a fortnight past. The pulse was about 100. The whole of this side of the chest emitted a dull sound, and respiration was indistinct, except at the spine and in the infra-mammary region. Can only lie on the side opposite to that where the swelling is. At 3 o'clock on the morning of the 5th February, she began to expectorate, by an incessant short cough, a quantity of purulent matter, which more than filled a wash-hand basin. The cough and expectoration in the forenoon of that day were not so troublesome, the swelling was much gone, and a gurgling sound was heard when the hand was laid on it. Expiration and inspiration were very loud to the ear applied over the part by the intervention of the stethoscope. Fluid could be pressed from below the external parts into the cavity of the chest, and the point of the finger could close a small opening felt betwixt two of the ribs, thereby preventing the blowing out of the external integuments in respiration. Much heat of skin, thirst, pulse 100. The sputa without odour, sometimes tinged with blood, or of a reddish or pinkish colour. On the 8th, the expectoration was much less, but gurgling sound still perceptible. On the 12th, the cough and expectoration had ceased, and she felt easy. No swelling on the side, but a rushing sound still heard by means of the stethoscope. Pulse 72; appetite good. On the 18th February she was discharged perfectly well.

This woman had lost one of her family by phthisis a little before she entered the hospital; and she was known to be strongly predisposed to that disease, and to have had premonitory symptoms of it previous to her attack of fever. It did not appear to me, however, that the collection of fluid in the pleura had any connection with that disease. It has been no unusual circumstance, during the present epidemic, for attacks of erysipelas to take place in the fever patients in the hospital; and a very frequent *sequela* of that disease has been large collections of purulent matter in some part of the body, generally in some superficial part, or numerous small abscesses. Such was the case with the abscess in the back in Bate; and the collection in the pleural sac I regarded as of the same nature, as rather a deposit, than the result of severe inflammatory action, for such never did exist; the deposit being directed to this part, no doubt, by the previous pneumonia or pluero-pneumonia. In the *post mortem* examinations of several cases of fever, we found an extensive collection of pus in the pleura of one side; and in many of these the common symptoms had not previously led us to suspect any inflammatory disease in the chest. The continuance or occurrence of dry tongue, heat of skin, want of appetite, and quick pulse, after the external symptoms of erysipelas had gone off, led us to suspect some collection of pus without local pain, and we were seldom wrong. Where no abscess, or internal collection of pus, could be discovered, we generally found the urine containing a quantity of pus; and these cases were often very tedious in recovery. Mrs Bate has had occasion to be several times at the hospital on account of her family. She is quite well; has no cough; and in the chest nothing abnormal can be discovered by percussion or the stethoscope.

III. *Epilepsy and Apoplexy.*—The following case is in no respect very remarkable, excepting that the *post mortem* examination explained most satisfactorily the cause of epilepsy, and in what way the case had proved fatal by the symptoms of apoplexy.

John Ireland, aged 52, a pensioner, of sober and industrious habits, had been subject for above seven years to epileptic fits at uncertain intervals. He had no fixed pain or uneasiness in his head; and his mental faculties were perfectly sound. On Tuesday the 12th August, whilst proceeding to put on his coat in the workshop, before going to dinner, he fell down in a fit. As this was not unusual, he was not interfered with; but when he had lain insensible for a much longer time than customary, assistance was sent for, and he was found in a comatose state with dilated pupils, stertorous breathing, and occasional spasmodic motions, until seven in the evening, when, after a convulsion, he died.

Not being in town I did not see him alive, but, having frequently seen him before, I was anxious to examine the head, which was done thirty-six hours after death.

Before dividing the scalp, a contusion was observed on the right temple, extending to the eye of the same side, probably, it was supposed, from his striking some bench or stool, or such object in the workshop on his falling. On removing the skull-cap, the *dura mater* over the middle lobe of the right side, and partly also over that of the posterior lobe, was of a dark blue colour, and the finger at once discovered fluid under it. On cutting it open, it was found that there were from two to three ounces of blood, mostly coagulated, lying betwixt it and the brain, and extending downwards into the middle depression in the base of the skull. The anterior part of the middle lobe, where it rests in the hollow of the bone, was rather softened and covered with much blood, and a rent or fissure was discovered extending from the squamous plate of the temporal bone across in an oblique direction the sphenoid bone. The sphenoid bone from below the transverse process to the spinous process was rough with several elevated spicula, some of which were as sharp as needles. From the quantity and situation of the blood it appeared as if the spinous artery had been the seat of the rupture.

On the anterior and upper part of the left hemisphere, about two inches above the orbit, the finger when drawn across the *dura mater* detected a hard flat substance under it; and it was discovered that there was at this part on its under surface a plate of bone of about one inch long and half an inch broad, smooth on its upper surface where it adhered to the internal part of the membrane, but irregular on its under surface, which was imbedded in the cerebral substance, and part of the latter, in rather a softened state, adhered so firmly to it that the bone could not be removed without bringing part of the brain with it. The arachnoid and *pia mater* adjacent to this part were thickened. No other disease could be discovered in the head.

ART. VII.—*On the Cure of Strabismus.* By THOMAS ELLIOT, M. R. C. S. L., Carlisle.

IT is now upwards of five months since I forwarded to the editor of the Lancet* an account of three cases, where the division of the internal rectus of the inverted eye failed in removing its squint. In these cases, the inversion was slightly diminished, but still confined to the eye that had been operated upon, while the patient was directed to look at an object placed before him, both eyes being uncovered.

But, in the same paper, I stated that I drew the following inferences from the cases above alluded to :—

1*st*, " That the strabismus could not be regarded as confined to one eye, since, when the sound eye was closed, the affected one became straight, and could move in any direction; but, on raising the lid of the former, it was found inverted, though the position of the eyes was soon reversed again."

2*d*, " That the straight position of the sound eye appeared to me to depend on the state of its optic nerve, as vision was much more distinct with it when the squinting eye was closed, than *vice versa*."

3*d*, " That the convergence was mutual, as was seen by the inversion of the sound eye, when (after closing it) the eyelid was quickly raised."

And 4*th*, " That the return of the sound eye to the centre of the axis of the orbit must be the result of a muscular effort, —an effort sufficient to overcome the contraction of its undivided adductor, and made to accommodate its more distinct vision."

On reading over the opinions of different writers up to that time, I found that the affection had been regarded as confined to, because apparent in the inverted eye; and that, in consequence of such cases being looked upon as those of single squint, the operation or operations had been practised on one or more muscles of the same eye.

Thus, in a paper written by W. R. Ancrum, Esq. (vide Lancet of July 18, 1840,) in a summary of seventy-six operations, Mr Liston is mentioned as having " partially or completely divided the inferior rectus in about ten cases," where the patient possessed the power of turning the eye a little inwards and downwards.

Again, in an appendix to his System of Practical Surgery, Professor Lizars, (page 4), mentions the necessity of adopting

---

* Vide Lancet of September 19, 1840.

Fig. 1.

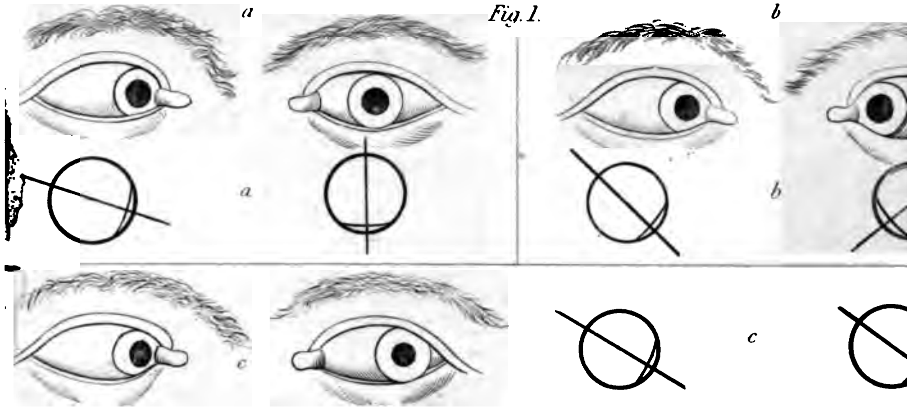


Fig. 2.

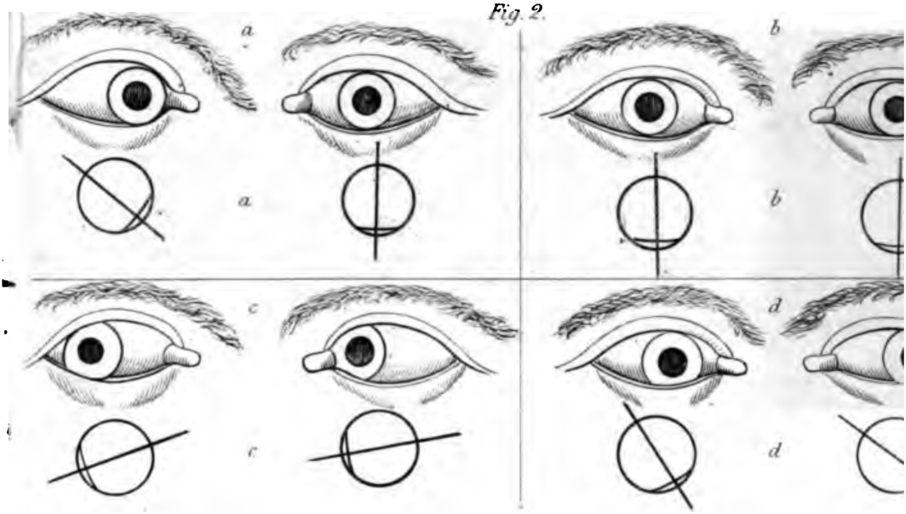


Fig. 3.

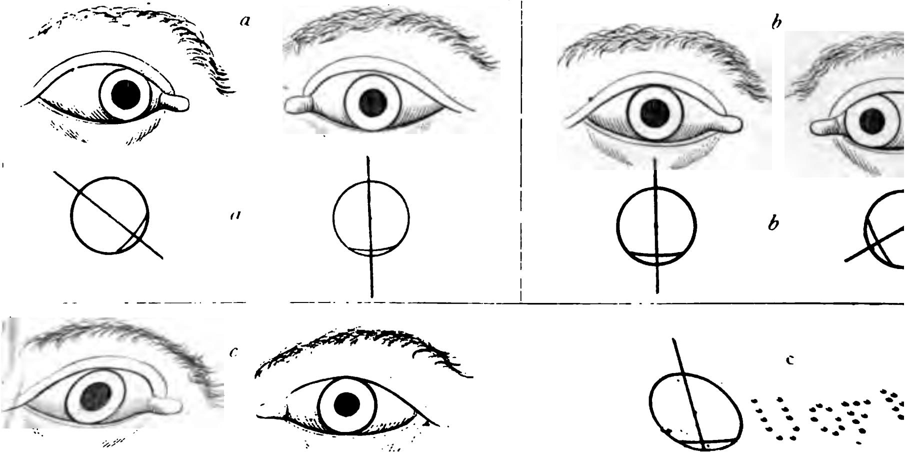


Fig. 1.

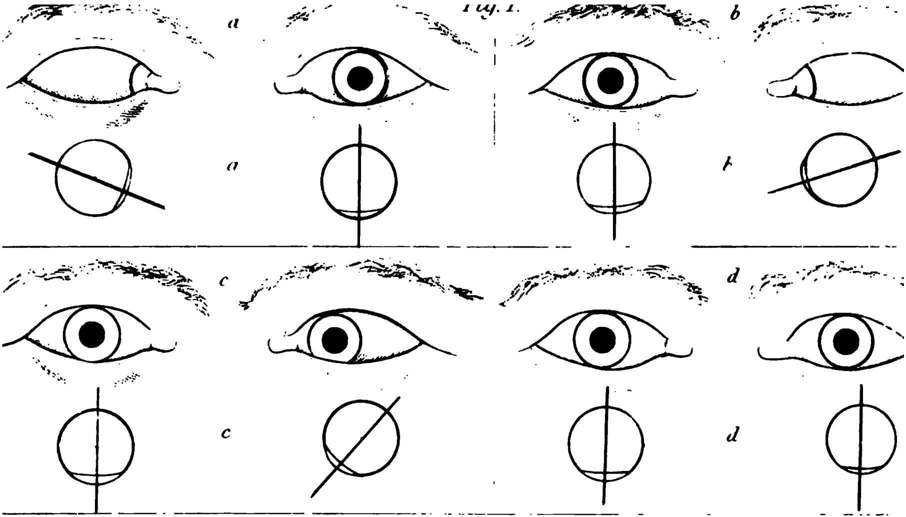


Fig. 2.

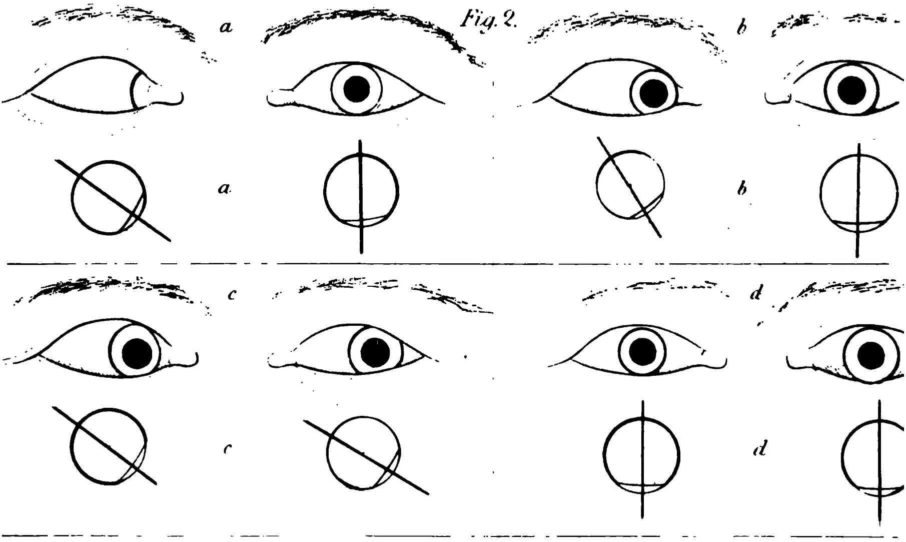
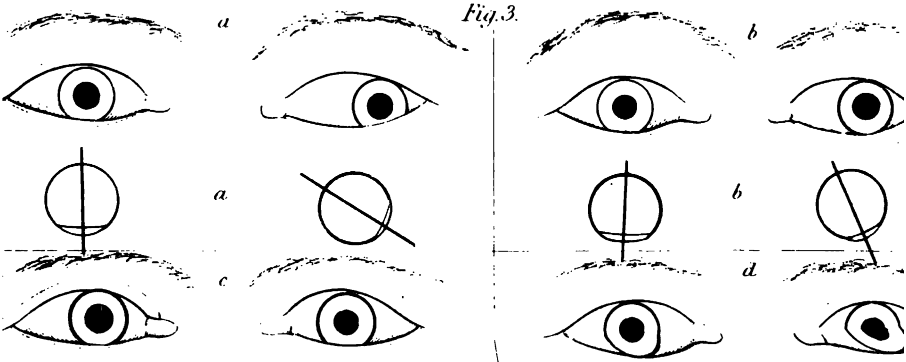


Fig. 3.



the same practice in inveterate strabismus of one eye, where the division of the adductor and of its contiguous fibro-cellular tissue has failed in removing the eye's power of inversion.

In a paper, dated September 1, (vide Lancet of September 12), Mr Duffin, in alluding to Mr Liston's practice, says, " he does not by any means call in question the fact, that in some few instances it may be necessary to divide the inferior rectus, though, in the same paper, he tells us that he has always found, on careful examination, that a few tendinous bands remained unseparated; and on dividing them, the patient could no longer carry the pupil beyond the centre of the orbit towards the nasal canthus.

In another paper, dated September 16, (vide Lancet of September 26, 1840), Mr Duffin says, " When the adductor muscle is cut across, and the eye liberated completely from any adventitious or other attachment that may still retain it in its unnatural position, the abductor muscle, if in a healthy condition, immediately draws the pupil to the visual centre of the orbit;" thus attributing the remaining inversion, if any exist, to an unhealthy state of that muscle.

Other surgeons have been satisfied with having removed the greater part of the deformity, by the division of the adductor, and preferred allowing a squint to remain, to dividing more muscles of the same orbit, and substituting an unsightly prominence for the original squint, (vide case of C. Culbert, by Mr Lucas, in the numbers of the Lancet for April 18, May 2, and July 4, 1840.)

At the 16th page of the fourth edition of the Report of the Royal Westminster Ophthalmic Hospital, by C. W. G. Guthrie, Junior, dated September 18th, the following remarks are made on the causes and treatment of the remaining inversion in such cases. He says, " In some rare and very obstinate cases, the eye, almost immediately after the division of the muscle, returns to nearly its pristine state of obliquity, and these are, I presume, the kind of cases in which it has been thought necessary to divide, or to attempt to divide other muscles than the one originally intended to be operated upon; and although the division and separation of the muscle and its attachments, in the manner I have especially indicated, in every direction, until the outer or sclerotic coat is fully exposed to a considerable extent, enables us to succeed, in most instances, in effecting the object in view, it does not always do so, and, in a few cases, I have been obliged to desist, without bringing the eye to an exact central position; or I have found it a little turned inwards or outwards on the next, or some other succeeding morning. The remedy for the evil is to exercise the eye in a regular manner."

Mr Guthrie then says, "The cause of the difficulty I believe to be twofold. It may occur when the incision in the conjunctival membrane unites, in the first instance, by adhesion, which it will do in some cases when it is small, and the eye by turning at all towards its former oblique state, facilitates this process. I suspect, also, that the ball of the eye rests in its peculiar bed of membrane and fat, like a cast in a mould; and that when the eye is liberated from its squinting state, it does not remain in its new and partially unsupported condition, but gradually falls back into the mould of fat and membrane originally formed around it. I am led to apprehend, that the conjunctival membrane materially assists, from its general attachment to the eyelids, in maintaining the eye in its old position, from which it is, however, gradually drawn, in these cases, by the efforts of nature, assisted by art."

In a letter recently addressed to the Academy of Sciences, (vide Lancet of July 11, 1840), Mons. Guerin states, that the results of his operations "have been advantageous, but not immediately so. In one case only did the eye become quite straight soon after the operation; in the others, there was merely an amelioration, and this circumstance appears to me to be a natural consequence of the true origin of squinting."

The operation was mentioned in a late number of the Provincial Medical and Surgical Journal, as being equally unsuccessful in the hands of MM. Roux and Velpeau of Paris, who, however, did not recommend any plan for the removal of the remaining deformity, but regarded the operation as one that would shortly fall into disuse.

The opinions first advanced by myself, as will be seen on referring to the inferences quoted at the beginning of this paper, were, that in cases where there was an association in the movements of the eyes, the affection could not be regarded as confined to one eye; and that the circumstance of the same eye still continuing inverted, after the division of its adductor, simply depended on two things, viz. 1*st*, The mutual convergence not being removed; in other words, the parallelism of the optic axes not being restored; and 2*d*, The effort made by its superior and inferior recti to invert the eye already operated on, in order to accommodate the more distinct vision of the other or sound eye.

The practice of dividing the corresponding muscle of the straight or sound eye, as it is generally termed, (adopted in the month of August 1840), was undertaken in order to remove the remaining mutual convergence, and restore the parallelism of the eyes; and its satisfactory result was mentioned in the particulars of two cases given in the same paper, when the inversion was still confined to the eye that was first operated on.

It was adopted in preference to the division of the superior or inferior rectus, which, though it might restore the parallelism of the eyes, and remove the squint, would leave a prominence of the eye that was operated on, and limit its future movements; and in preference to the "exercising of the eye in a regular manner," which, though it might succeed in some instances, after a practice of weeks, or even months, in rendering the first eye the straight one, by improving its vision, would fail in restoring the parallelism of the eyes, and only change the inversion from the one eye to the other, and never succeed even in that, in cases where the vision of the first eye could not be improved to the necessary degree by its change of position.

The best definition of strabismus that occurs to me is—that state of the eyes in which parallelism of the axes of the globes, in their different movements, no longer exists.*

Strabismus, as thus defined, may be conveniently divided, for practical purposes, into two great classes, viz. 1. Those cases in which there is no association whatever in the motions of the two eyes; or which only can be properly called cases of single squint; and, 2. Those cases in which there is an association in the motions of the eyes, where the position of one eye is always dependent on that of the other, and which may, with equal propriety, be called cases of double or mutual squint.

1st Class. Cases of strabismus in which there is no association in the motions of the eyes, or cases of single squint.

The affected eye may be either fixedly inverted or everted. No change of position takes place in it on closing the other eye, or on directing the patient to look to one side or the other, when both eyes are open. In this class there is inversion or eversion of one eye, but no mutual convergence or divergence; and the deformity may be caused either by paralysis of the opposite muscle, or morbid adhesions.

In my own practice I have only met with one example of this class, which I shall briefly give.

John Williamson, aged 14; squint of ten years duration; cause, an injury of the head. On looking at an object placed before him, the position of the left eye was natural, while the inversion of the right was so great as to conceal the inner margin of the cornea. (Vide Plate I. Fig. 1. *a.*)

---

* In a practical treatise of this kind, it would be out of place to enter into a history of the causes of such loss of parallelism; nor is it my intention to take any notice of the temporary forms of it, incident to childhood, which may be removed without having recourse to operation.

1. If the mutual convergence be slight, as seen by a corresponding slight inversion of one eye, when the patient regards an object placed before him, both eyes being open.

2. If the power of abduction be free, which I believe is always the case where the inversion is principally caused by the adductor, and not much shared in by the superior and inferior recti.

3. If the eyeball be large, as in that case it will be less acted on by the inner fibres of the superior and inferior recti, unless their tendons be large in a corresponding degree, so as to wrap over to the inner side of the ball, which I have not observed to be the case; and,

4. If the patient be young, or the squint of short duration, which diminish the probability of any great implication of the inner fibres of the superior and inferior recti.

There can be no doubt, that the parallelism of the eyes would be restored in these cases by the division of the adductor of either eye; but a preference must be given to that of the inverted or less clearly seeing eye, for the following reasons. Suppose the left eye to be usually inverted, or not used, on account of its indistinct vision. The section of the right adductor would, by restoring the parallelism of the eyes, remove the inversion of the left eye, and no squint would be perceptible on desiring the patient to look at an object placed before him; but the movements of the eyes, on looking to the right side, would be too free; while, on looking to the left side, on the contrary, the eversion of the left eye and inversion of the right would be limited in a corresponding degree, from the circumstance of the inner fibres of the superior and inferior recti of the former having been probably in some degree implicated or practised in the production of the almost constant inversion of that eye, so as to counteract more or less the abductor's action; and the same fibres of the other or right eye would probably offer but a feeble opposition to the right abductor, as the actions of the different muscles of that eye may be presumed to be normal, in consequence of its natural movements when both eyes are open, and it is made use of.

In cases, however, of this kind, where the sight of the eyes is equally distinct, and where, consequently, they are made use of alternately, the operation on either eye will answer equally well.

Case I.—September 13. Elizabeth Dixon, aged 19. Inversion of the left eye since her birth; position and motions of each eye natural, when the opposite one is closed. When the left eye is straight, the right is seen inverted on raising its lid suddenly. When both eyes are open, and the patient looks before her, the inversion is always seen in the left. Its vision is indistinct; eyeballs large. A peculiar rotatory motion is constantly seen in both eyes, probably caused by an irregular action of the two oblique muscles.

Operation.—The patient being seated on a chair, a table-cloth was passed round her arms and body, and secured behind its back. Her head was allowed to lean against the breast of an assistant, who at the same time raised the left upper eyelid by means of a speculum, the right eye having been previously bandaged. The patient was then desired to evert the eye as much as possible, the lower eyelid being at the same time depressed by the forefinger of my left hand. One blade of a pair of small curved eye-scissors was then passed through the conjunctiva, about three lines internal to the cornea, and a little below the inferior border of the adductor, and pushed gently upwards between the conjunctiva and sclerotic to the extent of four or five lines, when the former was divided. A blunt hook was next passed through the wound of the conjunctiva, its point being directed from below upwards between the tendon and the ball. A second blunt hook being introduced was pushed from before backwards, so as to separate completely the muscle from the ball, the first hook serving to steady the eye. On withdrawing the second hook, the tendon was brought gently forwards into view by means of the other, and divided by a pair of common angular scissors.

Result.—On removing the bandage, the eyes were observed to be quite parallel, though for a few seconds the left one appeared to be slightly everted.

There was a temporary relapse, which soon disappeared, and the eyes are now (February) quite parallel. The rotatory motion round the axis of the balls has also disappeared.

Remarks.—The mode of operating, as above described, was adopted in all my cases. By keeping the convexity of the curved scissors towards the cornea, and point consequently from it, the risk of injuring the ball is much lessened. I have never found it requisite to raise the conjunctiva by means of forceps or hooks, previous to making the necessary incision. By dividing that membrane near the cornea, the protrusion of cellular tissue is avoided, as well as the flow of a few drops of blood, which seldom appear till the tendon is cut through, thus enabling the operator to see clearly the different steps of the operation. Another advantage is the absence of all chance of permanently injuring the appearance of the *caruncula* and *plica semilunaris.* In some few cases it was found necessary to make use of a sharp hook, inserted into the sclerotic, to evert the eye to the necessary degree. Considerable difficulty is occasionally experienced, in consequence of the violent spasmodic contraction of the orbicularis, which forces the lining membrane of the upper eyelid downwards, and of the lower one upwards, so as completely to cover the eyeballs, in spite of all attempts to bring it into view by raising the former and depressing the latter. With a little patience, however, and perseverance, this difficulty has been always overcome. In operating upon children, it is advisable to secure the legs and feet to the chair; and in case the eye be forcibly inverted, so as to conceal the white, it may be drawn outwards by inserting the sharp hook into the sclerotic external to the cornea.

The operation, as thus described, I have frequently performed in fifteen seconds; but it sometimes happens that the whole of the tendon is not raised on the blunt hook. In such cases, the hook must be reintroduced, and a careful search instituted, sweeping from below upwards, over the posterior and inner surface of the ball, and then drawing it forwards, and cutting through any slip of the tendon, or cellular tissue previously undivided.

If the patient is unable invert the eye beyond the centre of the orbit, the operation is complete as regards that eye; and this has been mentioned by different authors as the only test of the operation having been perfectly performed. In the great majority of cases, I have found, however, that the eye could be inverted considerably beyond that point, but not so as to conceal all the white of the eye, or the inner margin of the cornea, though the complete division of the tendon and cellular tissue connecting the muscle to the ball was proved in some of these cases by the clean exposure of the inner surface of the globe, and in all by a careful search with the blunt hook.

Case II.—Sept. 14. Miss Huggins, aged 19. Strabismus congenital. On looking at an object placed before her, the left eye was inverted, and the right central or straight. On closing the latter, their position was reversed, as was seen by raising its lid. Power of abducting the left perfect; eyeballs large.

Operation.—The left adductor was separated and divided as described in the last case, and the cure was instant and perfect, both eyes being straight.

Remarks.—The patient, who was deaf and dumb, was made to evert the eye by signs. There was not the slightest appearance of blood till the tendon was divided. The power of inverting the eye after the operation was considerable. It is now five months since the operation was performed, and there has never been the slightest relapse.

Case III.—Sept. 24. Robert Forster, aged 17. Squinted for eight years. On regarding objects in front of him, the right eye was always inverted, and the left straight. On closing the latter, their position was reversed, the left being seen for an instant inverted on suddenly raising its lid. Abduction of the right free, its cornea nebulous, and vision indistinct.—(Vide Pl. I. Fig. 2. *a.*)

Operation.—On dividing the right adductor, the eyes were instantly made parallel.(Fig. 2. *b.*) on looking forwards.

Remarks.—On looking to the right side, the left cornea was partly concealed, and a slight convergence was produced, (Fig 2. *c.*) On looking to the left side, a slight divergence was apparent, from the incomplete power of inversion of the right eye. (Fig. 2. *d.*) The patient was directed to practise looking to the left side, to prevent any recurrence of the mutual convergence, till the eyes were accustomed to their new association of movements. The cure is still perfect.

Case IV.—Sept. 29. Miss W., aged 23. On looking at objects placed before her, the right eye is straight, and the left much inverted. When the right is closed, their position is instantly reversed, as seen by raising its lid. Perfect power of abduction of the left eye, but its vision very indistinct; eyeballs larger.

Operation.—The left adductor was separated and divided, and the eyes were instantly made parallel on looking forwards.

Remarks.—In three weeks there was a relapse, or renewal of the mutual convergence, and inversion of the left eye; but by practising looking to the right side, the squint has disappeared. In order to render the cure permanent, the patient is directed to bandage the right eye occasionally, so as to improve the vision of the left to the necessary degree.

The next case, and last of the kind which I shall give, is particularly interesting.

Case V.—Oct. 10. James Wilson, aged 16. Strabismus congenital; small part of the white of the inverted eye seen internal to the cornea when the opposite eye is straight. One eye always inverted on looking forwards, but not always the same eye, as they are used alternately; vision of the right eye rather more distinct, though good in both. The inversion is often equally divided, when looking at objects very near the face. Power of abduction of both eyes perfect.—(Vide Plate I. Fig. 3. *a* and *b.*)

Operation.—The left adductor was separated and divided, and perfect parallelism instantly restored. (Fig. 3. *c.*)

Remarks.—This case would have been called one of double squint by the most careless observer. The patient made use of the eyes alternately; and the only reason for selecting the left eye for operation was the slight difference of distinctness of vision. The motions of both eyes were free, and the alternate inversion was completely removed by the operation upon one eye.

The preceding cases will serve sufficiently well to illustrate this class, which, though single as regards the operation, are in reality cases of mutual convergence, as is proved both by the dependence of one eye on the other as to the position before the operation, and by the effect of the division of one adductor, not only on its own eye, but also on the other which is no longer inverted, when its lid is closed and raised.

The only difference between these cases, and those to be next described, is in the degree of convergence, which, if great, will require the division of the second principal converging muscle, or the adductor of the other eye, whether that convergence be indicated by a continued inversion of the same eye after the division of its adductor, or the inversion be apparent in the second or formerly straight eye.

Before entering into a description of the next variety, however, it may be as well to mention the causes of relapses in such as have been described, and the practice to be adopted for its removal.

If an occasional inversion of the less clearly sighted eye be seen on desiring the patient to regard an object placed before him, and if it be removed by closing the other eye, which in its turn does not become inverted, as seen by raising its lid, the fault must be attributed to the indistinct vision of the former eye, in consequence of which an effort is made by its superior and inferior recti to prevent its interference with the clear-sighted eye, and to avoid the confusion that would otherwise arise from regarding objects with eyes whose powers of vision were different. In such cases, the eye, whose adductor has not been divided, must be bandaged; and the other eye must be regularly practised, till the sight of both be made to correspond, when the cure will be complete and permanent.

It often happens, however, that a recurrence of the old evil, or mutual convergence, takes place, as indicated by the change of position of the eyes, on closing the sound or clear-seeing one, which may be seen slightly inverted on raising its lid. The relapse is to be here attributed to the continued influence of the former association of the motor nerves, which causes an increased action of the remaining converging muscles; and is to be removed, not by bandaging either eye, which has no effect on the convergence, but only substitutes the inversion of one eye for that of the other, but by directing the patient to look to that side which will cause even a slight divergence, viz. to the left side, if the right adductor has been divided, and *vice versa.*—(Vide Plate 1. Fig. 2. *d.*)

The reason for this is obvious, as the power of inversion of one eye, after the division of its adductor, does not exist in a corresponding degree to that of eversion of the other; and the success of the practice is another strong proof of the proper distinction between single inversion, and mutual convergence indicated by inversion, as the plan just mentioned would be the most likely one possible to increase the original evil, were it really, as is too generally thought, confined to the inverted eye.

b. Cases of Mutual Convergent Strabismus requiring the division of both adductors.—In this division I include, 1*st*, those cases in which parallelism of the eyes is not perfectly restored by the division of the first adductor; and, 2*d*, those cases of relapse where the convergence cannot be removed by practice, whether it be apparent by a renewal of the inversion of the eye already operated on, or by a change of inversion from the first eye to the other, which inversion, as I have before mentioned, will be found to depend on the relative powers of vision of the eyes.

I shall first give a few examples where the inversion was removed from the eye whose adductor had been divided, and appeared in the other eye.

Case I.—October 10. John Batey, aged 22. Strabismus of seven or eight years' duration. Inversion of the right eye so great as to conceal the inner half of the cornea, when the left eye is central, or looking at an object placed before the patient. He occasionally uses the right eye, when the left one is as much inverted as the right generally is. The sight of the left eye is most distinct. Abduction of neither quite perfect.

Operation.—The right adductor was separated and divided, when that eye instantly became straight, and a slighter inversion appeared in the other or left eye. In a few minutes the left adductor was separated and divided, and parallelism on looking forwards perfectly restored.

Remarks.—The similarity between this case and that of James Wilson before described must strike every one. In both, the eyes were used alternately; the only difference being in the degree of mutual convergence, indicated by the different degrees of inversion.

The circumstance, in this case, of the inversion leaving the right eye and settling in the left, after the first operation, is accounted for by the tolerably distinct vision of the former, which prevented the necessity of any effort being made to accommodate the other eye.

Case II. October 13. Robert Wilson, aged 12½.—On looking at an object in front of him, the right eye was straight, and the left eye so much inverted as to conceal all the white internal to the cornea. When the right eye was closed, their position was reversed. The left eye could scarcely be everted beyond the centre of the orbit. Its vision was indistinct.

Operation.—The left adductor was separated and divided, and the remaining convergence was indicated by an inversion, sometimes of the right eye, at other times of the left.

The right adductor was then divided, and the cure made perfect. There has never been the slightest relapse.

Remarks.—The probability of a second operation being required, was indicated in this case by the limited power of abduction. The change of, or rather alternate inversion seen after the division of the first adductor, depended on the object looked at. If small, the right eye continued straight; if large, and such as could be seen distinctly by even the left, the right eye in its turn was inverted, as less effort was of course required for that position of the eyes.

Case III. October 13. Ann Smith, aged 9.—Squint of five years' duration. On looking at an object placed before her, one eye is always straight, and the other so much inverted as to conceal the white, internal to the cornea. The eyes are used alternately, though the right is most frequently so, as its vision is rather more distinct than that of the left.

Operation.—The left adductor was separated and divided, when that eye became straight, and the slightest possible inversion appeared in the other. The patient was directed to practise regularly as before explained; but the convergence, indicated by an increasing inversion of the right eye, continued to gain ground.

As the left eye was rendered rather prominent by the operation, the patient agreed to submit to the division of the second adductor, which was done on the 30th of October 1840. The instant the muscle was divided, the eyes were made perfectly parallel, and corresponded exactly in appearance. There has been no second relapse.

Remarks.—The relapse that supervened on the first operation could only be attributed to the influence of the ill-directed nervous association that regulated the motions of the eyes, and caused an increased action in the remaining converging muscles, and inversion of the clearer-sighted eye.

Case IV. October 16. Mrs Liddell, aged 31.—Squinted since childhood. One eye always much inverted on looking forwards; generally the left one. When the right is straight, the inner margin of the left cornea is concealed. Abduction of the latter nearly perfect.

Operation.—I separated and divided the left adductor, and the eye was still slightly inverted. On searching with the blunt hook, some fine cellular tissue was drawn forwards and divided. A slight inversion then appeared to settle in the right eye. The right adductor was then divided, and parallelism instantly restored; the eyes at the same time being rendered equally full. There has been no relapse; and the sight, as in all the previous cases, has been much improved.

Case V. October 19. Mrs Gaddes, aged 33.—Squint of twenty-nine years' duration. Alternate inversion of the eyes. Sight and size of pupils correspond. If the right eye was directed straight upon an object placed before the patient, two-thirds of the left cornea were buried in the inner canthus; and on directing the patient to close the eyes for an instant and re-open them, their position was exactly reversed. This experiment was frequently tried, and always with the same result. In short, no difference could be detected between the eyes, as they seemed to relieve each other by acting alternately. (Plate II. Fig. 1. *a* and *b*.) The eyeballs were large, and their powers of abduction nearly perfect.

Operation.—The right adductor was separated, and its tendon divided. The right eye was still occasionally inverted. On reintroducing the blunt hook, a slip of the tendon was found undivided, and cut through. The inversion, though in a much slighter degree than before the operation, was then seen to settle in the left eye. (Vide. Fig. 1, *c*. Plate II.) The left adductor was next separated and divided, and on uncovering the other eye, both were seen perfectly straight. There has never been the slightest relapse up to this time. (Fig. 1, *d*.)

Remarks.—This was the most beautiful case of mutual convergence and alternate inversion that I have seen. The effect of the division of the right adductor was seen, not only on its own eye, but also on the left one, as seen by its diminished inversion. That both adductors would require division was evident before the operation, from the very great alternate inversion, and the duration of the squint. And that no relapse would take place was rendered probable, 1*st*, from the facility of regarding objects with both eyes, as their vision was equally distinct; and, 2*dly*, from the difficulty that the superior and inferior recti would experience in causing that relapse, in obedience to the long-continued and ill directed association of the motor nerves, on account of the large size of the eyeballs, which, as before-mentioned, are little acted on by those muscles, unless their tendons are large in a corresponding degree, so as to wrap over to their inner sides.

Case VI. Oct. 26.—Mrs Mary Cullen, aged 40. Squint of thirty-eight years' duration; right eye generally inverted; the inner half of its cornea being concealed when the left is straight. Their position is reversed when the right is made use of. Abduction of each eye nearly perfect.

Operation.—Separated and divided the right adductor tendon, when a slight inversion appeared to settle in the left eye. The left adductor tendon was then merely divided, without detaching the muscle from the ball, for fear of producing eversion, and parallelism was perfectly restored. There has been no relapse.

These cases will be found in every respect the most satisfactory, as a relapse seldom supervenes; and when it does, is easily removed by practice.

The patient must be directed to practise looking to either side, if a slight tendency to convergence be apparent, till the eyes are accustomed to their new association of movements, or association of parallelism. By this side practice, a slight temporary divergence is even produced, which is caused, not, as has been stated, by an undue action of the abductor, but by the inability of the other eye to invert in proportion, from the division of its adductor. This, however, soon disappears, and the eyes correspond perfectly both in appearance and movements. On dividing the second adductor, I have occasionally seen a slight divergence produced, even on looking forwards; but this has always been removed in the course of a few days. The patient in such a case must be directed to look forwards at objects, and avoid side-practice.

Cases are recorded, and I know of two in this neighbourhood, where the divergence produced was great, and could not be removed by practice. In such circumstances the division of either abductor will answer equally well in restoring the parallelism. But where the divergence has been produced by the division of only one adductor, I should select the other eye for the division of the abductor, in order to render them equally prominent. I may mention, however, that such an unfortunate result has never taken place in my own practice, and I believe it may always be avoided by first merely dividing the tendon, if the convergence be slight, and then, in case it be not removed, by reintroducing the blunt hook, and separating the muscle from the ball to the necessary extent.

I shall now give a few examples of the last variety of cases belonging to this division, viz. those where a still continued convergence, after the division of the adductor of the inverted eye, is indicated by a continued inversion of the same eye.

This continued squint or inversion, which is still confined to the eye whose adductor has been divided, when both eyes are uncovered, and the patient looking before him, has been attributed, (as will be seen by referring to the various quotations at the beginning of this paper,) to very different causes, such as an undue contraction of the inner fibres of the superior or inferior rectus, an unhealthy state of the external rectus or abductor, healing of the wound of the conjunctiva by first intention, the eyeball returning to its bed of membrane and fat, like a cast in a mould, an imperfect division of the adductor and its cellular tissue, &c. &c., and the practice adopted for its removal has varied according to the views entertained by those writers as to the reasons of the continued inclination inwards of the eye.

The remaining inversion is no doubt effected by the contraction of the inner fibres of the superior and inferior rectus; but, as I stated in the Lancet of September 19, this contraction is made to accommodate the more distinct vision of the other eye, and is not the result of a shortening of those fibres, as is at once proved by the reversed position of the eyes, when the straight or sound one is closed.

An unhealthy state of the abductor, contraction of the conjunctiva, &c. are also disproved by the same simple experiment.

The cause of the evil is the non-removal of the mutual convergence, the continuance of which is to be attributed to the ill-directed association of the motific nerves; and the continued inversion of the eye operated on, depends, as before stated, on its very indistinct vision, which renders it comparatively unfit for use.

Were the affection confined to the inverted eye, and the inclination inwards attributable merely to a temporary effort made by its superior and inferior recti to prevent confusion of vision, the practice of bandaging the sound or straight eye would be all that was required. But, as already mentioned, the only effect of that practice in these cases is reversing the position of the eyes, so that the utmost to be gained by its adoption would be a permanent change of inversion from one eye to the other by producing a change in their relative powers of vision.

I trust the distinction thus made between inversion and convergence will be clearly understood. The former depends on the eye's comparatively indistinct vision; the latter, on an abnormal association of the nerves of motion. That the fault lies in the nerves which regulate the movements of the muscles, and not in the muscles themselves, is evident, 1*st*, from the natural position of each eye when the opposite one is closed; and, 2*dly*, from the circumstance that a similar action is frequently adopted by the remaining muscles or superior and inferior recti, causing a temporary relapse, to remove which a side-practice of longer or shorter time will be often found necessary, till the eyes be accustomed to their new association or parallelism of movements.

It is on account of the difficulty of removing the converging tendency that I now invariably adopt the practice of dividing the second adductor, when an inversion, however slight, is seen in either eye on looking forwards, after the division of the first adductor; not to mention the improvement in the appearance of the eyes by rendering them symmetrical.

Hoping that these remarks will render the principles sufficiently clear, on which the practice of dividing the corresponding muscle of the straight or sound eye, as it has been frequently termed, was founded,* I shall proceed to give a few examples in illustration, begging leave to refer the reader to my paper in the Lancet of September 19, for the particulars of the successful results of the same practice in the two first cases of the kind in which I had an opportunity of adopting it, in both of which there was a temporary relapse, which was completely removed by side-practice. In the second of these the relapse could only be attributed to the motor nerves, and not at all to the nerves of sight or confusion of vision, as the inverted eye was affected with nebula, which rendered it utterly useless. Both cases are permanently cured; at least it may be presumed so, as there has been no convergence for several months.

Case III.—Sept. 14. Miss Rennison, aged 13. Squint of seven years' duration. Inversion of the left eye so great, as nearly to conceal all the white internal to the cornea, when the right eye was straight, and the patient looking at an object placed before her. On closing the right eye, their position was reversed, but on raising its lid, the left eye instantly turned in again. Right eye always made use of, as the vision of the left was very indistinct; abduction good; eyeballs small.

Operation.—The left adductor was separated and divided. On uncovering the right eye, a slight inversion was still apparent in the left. On searching carefully with the blunt hook, it was found that all had been divided. The adductor of the right and straight eye was then divided, which completely removed the remaining inversion of the left one, and restored the parallelism, at the same time rendering them equally prominent.

---

* A practice that has been regarded by one writer as "too absurd to notice," (Vide paper by Mr Clay, in Lancet of January 2, 1841;) and as a novelty, when adopted in *strabismus divergens*, calculated to produce incurable convergent strabismus. (Vide paper by Mr Duffin, in Lancet of November 7, 1840).

Remarks.—The continued inversion of the left eye in this case evidently depended on its indistinct vision, as it was nearly useless. The operation on the second and straight eye was undertaken in order to remove the mutual convergence, by depriving the motor nerves of their converging power. The power of inversion of both eyes after the operation was nearly natural, which might have been expected from the small size of the eyeballs.

In a few days, there was a slight occasional inversion of the left eye, evidently made to prevent confusion of vision, as it was removed by closing the other eye, which did not become inverted in turn, as seen on raising its lid, when both eyes remained straight. By bandaging the right eye for two or three weeks, the vision of the left was much improved, and parallelism restored.

Case IV. September 20.—John Dixon, aged 6. Very great inversion, and indistinct vision of left eye. Position of the eyes reversed on closing the right or straight one. Abduction of both good. Eyeballs large.

Operation.—The left adductor was separated and divided. On uncovering the right eye, the left eye again turned in. A careful search with the blunt hook showed that all had been divided. On closing the right eye, and raising its lid, it was seen slightly inverted, though only for an instant.

As the continued inversion of the left eye was thus proved to be the result, not of a voluntary effort on the part of its muscles, but of the non-removal of the mutual convergence, the adductor of the right, and still straight eye was divided, without separating the muscle from the ball.

Result.—Both eyes were perfectly straight on looking forwards.

Remarks.—Both eyes required operation in this case, on account of the great converging power, indicated by the great inversion of the left eye. As the eyeballs were large, it is probable that the tendons of the superior and inferior recti were large in a corresponding degree. The case is an extremely beautiful one, and there has never been any renewal of the mutual convergence.

Case V.—September 27, (Vide Plate II. Fig. 2, *a.*)—Mr Irving, aged 34. Squint of twenty-eight years' duration. Inversion of the right eye so great as to conceal the inner half of its cornea, when the left is straight. Right eye never made use of, as its vision is very indistinct. On closing the left, the position of the eyes was instantly reversed.

Operation.—The right adductor was separated and divided. On uncovering the left eye, the right one was still as much inverted as to conceal all the white internal to the cornea. On searching with the blunt hook, the eyeball's inner surface was cleanly exposed.—(Vide Fig. 2, *b.*)

After a little persuasion, the patient submitted to the operation on the left eye, in which no squint could ever be detected, except on closing and reopening it, and then only for a second or two.

Result.—Perfect parallelism and removal of the great continued inversion of the right eye.—(Fig. 2, *c* and *d.*)

Remarks.—For three days, the eyes remained straight, but at the end of that time, the right eye became slightly inverted again. In consequence of the patient's being obliged to attend to his business, (that of a barber), he was unable to bandage the left eye, on account of the bad sight of the right, which rendered it useless. The necessity of looking almost constantly forwards tended also to increase the convergence and inversion of the right eye, which at length became so great as nearly to conceal all the white internal to the cornea when the left eye was straight.

The patient, though anxious to undergo another operation, was induced to practise looking to the right side whenever he had an opportunity, and the result has been highly advantageous. When he now looks to either side, the eyes are parallel; and on looking straight forwards, they are frequently perfectly natural, though an occasional inversion, or rather cast, is seen in the right eye. I have little doubt but that this cast even will ultimately disappear, as the time devoted to side-practice has been short, in consequence of his almost constant occupation, which tends much to increase or renew the deformity.

Case VI.—September 28th. Mary Scott, aged 22. Inversion of the right eye so great as to conceal the inner margin of the cornea when the left eye is straight. Sight of the right indistinct. On closing the left eye, and desiring her to look at an object placed before her with the right, the latter remained inverted, and looked sideways at it, as the part of the retina corresponding with the foramen of Soemmering was not made use of. On moving the object looked at towards the right side, the eye became straight or central, and on then raising the left lid, the usual position of the eyes was seen reversed for a few seconds.

Operation.—The right adductor was separated and divided, but a decided inversion still appeared in the same eye, and entirely confined to it when both eyes were open, and the patient looking at an object placed before her. That the muscle was completely detached from the ball was ascertained by a careful search with the blunt hook. The adductor of the left and straight or sound eye was then divided, and the result was not only the removal of the inversion, but even the production of a slight eversion of the right eye.

Remarks.—The slight eversion appeared in the right or quondam inverted eye, as might have been expected from its indistinct vision, and showed the effect of the division of the left adductor in a remarkably distinct manner. Considerable difficulty has been experienced in this case in forming an association of parallelism in the different movements, as the eversion disappeared, and was followed by an inversion of the same eye. This has been overcome, however, by side-practice, as bandaging the left eye would have been worse than useless, on account of the insensibility of the centre of the right retina, and the constant inversion of that eye, on looking at an object placed before it. It is now upwards of four months since the operation, and on standing opposite the patient, and desiring her to look at you when both eyes are open, they are perfectly straight. On closing the left, the right eye instantly becomes inverted.

The parallelism is maintained in this case by the association of the nerves of motion, and not of sight, as the left eye alone is made use of when both are open.

Case VII.—October 10th. Joseph Colthwaite, aged 28. Left eye never used, and the inner half of its cornea concealed, when the right is straight, and the patient looking before him. On closing and reopening the right or straight eye, their position is seen reversed for a few seconds. Can abduct neither eye beyond the centre of the orbit. Vision of the left eye very indistinct.

Operation.—Separated and divided the left adductor. On uncovering the right eye, the left again turned in so much as to conceal the white, the right eye remaining perfectly straight. The right adductor was then separated and divided, and parallelism was restored as nearly as possible, though not perfectly.

Remarks.—In a few days, the cure was made quite perfect by side-practice, and there has been no relapse. The eyes now correspond both in appearance and powers of vision.

Case VIII.—Oct. 14. Mary Sheppard, aged 18. Squint of fifteen years' duration; inner third of the left cornea concealed when the right eye is straight, or the patient looking before her. On closing the right eye, their position is reversed, but on opening it, the left instantly turns in again. Vision of the left very indistinct. Power of abduction nearly perfect.

Operation.—The left adductor was separated and divided, but an inversion still remained in the same eye. A careful search with the blunt-hook proved the complete separation and division of the muscle and cellular tissue. The adductor of the right and straight eye was then divided, which completely removed the inversion of the left, and restored the parallelism.

Remarks.—A slight recurrence of the mutual convergence was soon removed by side-practice, and both eyes have since remained perfectly straight.

Case IX.—Oct. 22. Mr Gavin Gray, aged 23. Squint of eleven years. Inner margin of the right cornea concealed, when the patient looks before him, and the left eye is straight. Position reversed on closing the latter. Right eye never made use of, and its vision very indistinct; eyeballs large, and abduction of neither quite perfect.

Operation.—The right adductor was separated and divided, but the same eye still remained inverted. On searching with the blunt-hook, a little cellular tissue was taken up and divided. The inversion still continued. The adductor of the left and straight eye was then separated and divided, which restored the parallelism, and removed the inversion of the right eye, though the eyes appeared unduly prominent.

Remarks.—There has been no relapse. This is probably to be accounted for by the large size of the eyeballs, as the patient's business, that of a printer, was calculated to cause a recurrence of the mutual convergence. The cure is perfect, and appearance of the eyes quite natural. The vision of both also corresponds.

Case X.—Oct. 28. John Morgan, aged 20. Squint of eighteen years' duration. Inner margin of the right cornea concealed when the left is straight. Sight of right eye very indistinct. On closing the left eye, the right is not directed straight upon an object placed before it, but remains inverted. On moving the object to the right side, the eye becomes straight. By raising the lid of the other, their position is then seen to be reversed; abduction good; eyeballs very small.

Operation.—The right adductor was separated and divided, and the eyeball seen completely exposed. The same eye still continued so much inverted, as to conceal all its white internal to the cornea. The left adductor was then divided and separated, and parallelism restored, though occasionally a slight cast appeared in the right eye.

Remarks.—As was expected, much difficulty has been experienced in the cure of the relapse. The change of seat of sensibility of the right retina, and small size of the eyeballs, will at once account for this; the former rendering the inverted position of that eye most favourable for its use; the latter accounting for the great converging power of the inner fibres of the superior and inferior recti, which could invert the eyes by turns in a natural degree, immediately after the operation.

A few days ago, on examining the case, I found both eyes straight, but on closing the left, the right turned in as formerly, showing the association to be merely that of motion, not of sight.

Case XI. — Nov. 29. Matthew Robinson, aged 27. Squinted since childhood. On looking before him, the inner margin of the left cornea is concealed, while the right eye is straight. On closing the latter, their position becomes reversed. Sight of the left eye very indistinct, and a white speck seen in the lens. Abduction good; eyeballs small.

Operation.—The left adductor was separated and divided, but the eye still turned in. A careful search was made with the blunt hook, but it was found that all had been divided.

The patient was then urged to have the same operation performed on the other eye, but this he resolutely refused to submit to, as nothing could induce him to believe that it was at all affected. I thought it better to leave the eye inverted, than divide more muscles of the same orbit, and increase the prominence, which already existed after the division of the first adductor, though the patient was willing to undergo more operations on the squinting eye.

Side-practice, of course, proved in this case insufficient to remove the mutual convergence, and the eye still continues inverted.

Case XII.—Dec. 2. Miss Jane Robinson, aged 21. Squint of sixteen years' duration. On looking at an object in front of her, the inner margin of the left cornea was concealed, while the right eye was straight. Position reversed on closing the latter. Power of abduction very imperfect; vision much impaired, as the left cornea is nebulous.

Operation.—The left adductor was separated and divided. The patient could then abduct the eye perfectly. On uncovering the right eye, the left again turned in. As in all the previous cases a careful search was made with the blunt hook, but all was found divided. The adductor of the straight eye was then divided, and the inversion of the left completely removed. The cure was perfect.

Remarks.—A slight relapse was removed in a few days by side-practice. In this case, as in a few others of the kind on which I have operated, viz. where the cornea of the inverted eye was affected with nebula, the vision of the good eye was much improved. This can only be accounted for by the steadiness of its position, since it no longer requires any effort to prevent its inversion, the mutual convergence having been removed.

It would be useless to offer more examples of this variety of strabismus,—indeed, I fear they are already too numerous. But I wish to impress on the reader, 1. that the division of the adductor of the inverted eye very often fails in removing its squint; and 2. that its continued inversion may be removed instantly by a division of the corresponding muscle of the straight and clear-seeing eye, provided the convergence be mutual, as ascertained beforehand by their association of movements.

I am anxious to lay the more stress upon this, as I regret to learn that such eminent surgeons as MM. Roux and Velpeau regard the operation itself as unsatisfactory, and one that must soon be abandoned; an opinion entertained also by some surgeons of this country, who have operated on a number of cases, but confined their operation or operations to the inverted eye.

The continued inversion, as before-mentioned, depends on the eye's inutility for the purposes of vision; and the surgeon must direct his attention, not to the removal of that inversion by dividing more muscles of the same eye, but to the removal of the mutual convergence, in causing which the straight or non-squinting eye is equally implicated.

As the convergence, though not the inversion, is always shared by both eyes, so ought the operation to be, provided parallelism be not restored by the division of one adductor.

And in such cases, I would insist upon the division of both adductors at the same sitting, in order to remove the converging tendency as soon as possible, otherwise the habit assumed by the superior and inferior recti may be so powerful as to require operation on them also. *

In all the cases where I have divided both adductors at the same sitting, the squint has been removed. In most of them the eyes have been instantly made parallel on looking forwards, and equally so in a few weeks on looking to either side. In a few a slight divergence has been caused, accompanied by diplopia; but this has always disappeared in a few days. In two

---

* In one case only has the division of both adductors failed in removing the squint, and I can only attribute it to the habit acquired by the superior and inferior recti, as an interval of three weeks passed over before the second operation was had recourse to. A search was made at the same time to ascertain if the first adductor had formed a new attachment to the ball, but the latter was found quite clear. Though the person still squints, the effect of the division of the adductor of the straight eye was greater on the inverted one, than the division of its own adductor.

or three an occasional cast has been observed immediately after the division of both adductors, when an oscillation of the eye that was formerly inverted was seen, caused by the alternate action of the abductor, and superior and inferior recti, which soon disappeared when the eyes became accustomed to their new association.

Before proceeding to the description of *strabismus divergens*, I may mention that I have on several occasions, where the squint had been removed by the division of the first adductor, had recourse to the same operation on the second eye, in order to render them equally prominent, and make them correspond in appearance. The second operation in this case must be conducted with extreme care, so as merely to divide the tendon near its insertion, without separating its cellular attachments to the ball, for fear of producing eversion.

CLASS II. 2d VARIETY. *Mutual Divergent Strabismus.*—Like the last, mutual divergent strabismus may be classified under two heads, viz.—

a. Cases requiring the division of one abductor. *b. Cases requiring the division of both abductors.*

a. Cases of Mutual Divergent Strabismus requiring the division of one abductor.—Like the cases of mutual convergence, the closing of either eye makes the other straight; and on raising the lid of the closed one it will be seen everted.

When the mutual divergence is slight, as indicated either by the constant slight eversion of the same eye when the patient looks forwards with both eyes open, or by the alternate slight eversion of the eyes, when their powers of vision correspond or nearly so, the division of one abductor will be all that is required; and, for the reasons before given, I would select the usually everted eye, or that one whose vision is most indistinct, for operation.

Where, however, the eversion has resulted from the division of one adductor, and cannot be removed by practice, I would give a preference to the other or straight eye, in order to render them equally prominent.

If the eyes be made use of alternately, parallelism will be restored by the division of either abductor.

b. Cases of Mutual Divergent Strabismus requiring the division of both abductors.—The particulars of the first case of this kind that I operated upon (on October 6, 1840,) were given at sufficient length in the Lancet of October 31. The second case was alluded to in the same paper, and will serve to illustrate this class.

CASE. October 10. Plate II. Fig. 3. *a.* Mr Wilson, aged *41.*—Squint of thirty years' duration. On looking forwards, the right eye is always straight, and the left so much everted as to expose scarcely a line of the albuginea external to the cornea. The eversion is always seen in the left eye, its vision being so indistinct as to render it useless. On closing the right eye the left becomes straight; and on raising the lid of the former, it is seen everted for an instant, when their position becomes reversed. Power of inversion good.

Operation.—The left abductor was separated and divided, and was found scarcely thicker than a crow-quill. A slight eversion was still apparent in the same eye. On searching with the blunt hook, it was found that all had been divided. (Fig. 3, *b.*) After a little explanation, the patient readily submitted to the second operation.

The abductor of the straight or sound eye was then divided, which, by removing the diverging power,* and restoring the parallelism, removed also the continued eversion of the left eye. (Fig. 3, *c.* and *d.*) The cure was perfect, and the power of abducting each eye existed in the natural degree, immediately after the operation.

Remarks.—In a few days the left eye again appeared slightly everted, and on closing and reopening the other, the affection was proved to be mutual. The patient was directed to practise looking forwards at very near and small objects. By this means, a slight temporary convergence was produced, which was soon removed by the patient's judicious management. The eyes have now been perfectly straight for upwards of three months, and the vision of both now corresponds.

The other cases of *strabismus divergens,* on which I have operated, resemble this one so closely that it would be merely a repetition to lay them before the reader. In all of them, the vision of the everted eye was very indistinct, and consequently the inclination outwards after the first operation was still confined to it; and in all, the division of the corresponding muscle of the straight eye restored the parallelism.

The relapse that takes place in these cases requires considerable practice to remove, and in one of my cases a slight cast outwards still remains, particularly on looking upwards.

In none of them has there been any granulation, and this may probably be attributed to the horizontal incision of the conjunctiva, which does not leave the sclerotic so much exposed, the excrescence appearing to shoot from its surface.

When a granulation appears, it may be touched, if small, with nitrate of silver. If it be large, however, I wait till the wound of the conjunctiva contracts around, and partly strangulates it. The narrow neck is then snipped through with a pair of scissors, close to the bell, and the bleeding surface touched with caustic, which is generally found sufficient to prevent its renewal.

---

* I make use of the word power, for the diverging tendency or influence of the motor nerves exists even during the parallelism obtained by the operation, and generally shows itself by causing a temporary relapse.

In conclusion, I may be allowed to offer a few remarks on the classification generally adopted in cases of strabismus.

Mr Lucas (and in quoting his opinions I believe I lay before the reader those generally entertained) divides them into,

I. *Convergent*, II. *Divergent*, and III. *Anomalous.*—" Convergent strabismus," he tells us, " may affect one or both eyes ;" and his mode of ascertaining it is by holding his hand obliquely in front of the straight or better-sighted eye, and " desiring the patient to exercise the other." If, when the usually inverted eye becomes straight, the other retreats into the inner canthus, the case is one of double convergent strabismus, and will require the division of both adductors; " but if the contrary occurs, if both eyes are at this period straight, or even if the covered one has but a slight inclination inwards, the case is one of single convergent strabismus, and the inner rectus of one eye only will require to be divided." (Vide p. 48 and 49 of Mr Lucas's treatise on Squinting.)

Now I may again mention, that in all the cases I have seen, where the eyes were free in their movements, whether requiring the division of one adductor or both, the convergence was mutual, and the only effect of closing or shading one eye was substituting its inversion for that of the other. The convergence was neither removed by it, nor the eyes made parallel.

If parallelism can be temporarily induced by covering the clear-sighted eye, it ought to be bandaged till the sight of both is made to correspond, when a permanent cure, without having had recourse to operation, would be the result. Such a case I have not yet seen, and it could only be termed one of single inversion, not single convergence.

In cases requiring the division of both adductors, the mutual convergence is also shared by both eyes, and differs only in degree from those requiring the division of one.

In these cases, for the reasons already given, the inversion may be always seen in the same eye, when both are open, though the convergence is always shared by both eyes.

I have never yet seen the division of both adductors, when it was found necessary, fail in removing the squint, if both operations were undertaken at the same sitting. The relapse that generally supervenes has been perfectly removed in nearly all these cases by side-practice ; and where the vision of the eyes *can be* made to correspond, the cure will probably be permanent, though a sufficient length of time has not yet elapsed to render it certain.

The occasional cast that is sometimes seen, weeks or even months after the operation, may be always, I believe, attributed to one of two causes, viz. either a continuance of the converging tendency, in consequence of the necessary degree of side-practice not having been resorted to with sufficient perseverance on the part of the patient, or the difficulty of making the sight of the eyes correspond.

In the former case, I believe the cure may be made complete by the patient's good management.

In the latter, the propriety of removing the occasional squint by further operation is questionable, as that object can only be gained by permanently destroying the eye's power of inversion, and consequently depriving both of their beautifully associated lateral movements.

The preceding remarks and cases will show that much requires to be done after the operation. The converging or diverging tendency, (according to the kind of strabismus,) that exists during the parallelism obtained by operation, shows its effects in most cases on the remaining muscles by causing a relapse; and close observation on the part of the surgeon, with well-directed and persevering practice on the part of the patient, will be found requisite in order to render the cure complete.

With attention to these, however, the operation will not only be found one of the most beautiful, but also one of the most successful that was ever introduced into surgery.

Carlisle, 1841.

Art. VIII.—*Observations on Corpora Lutea.* Part III. *Containing the History of the subject.* By Robert Paterson, M. D., Vice-President of the Anatomical Society, Extraordinary Member of the Royal Medical Society, Member of the Obstetrical Society, and one of the Council of the Wernerian Natural History Society of Edinburgh, and Physician to the Leith Dispensary, Humane Society, and Casualty Hospital, &c.

In the former parts of these observations, it has been our object to describe the appearance of the true *corpus luteum*, not only of the human being, but of the lower animals;—the appearance of false *corpora lutea*, (as they have been termed,) and to point out the marks, which are to be looked upon as distinguishing these two, from each other. Our present inquiry is intended to include the history of the subject, as well as the conclusions to be drawn from it regarding the question of the existence of *corpora lutea* (properly so called, true bodies,) without impregnation,—the former parts being in fact practical and descriptive, the present being historical and explanatory. In examining the history of the subject, it will be our especial object to quote each author as fully as possible from his original works, and to add a few explanatory observations, so that each individual may judge for himself, from the quotations we shall make, if our explanations do not seem proper or sufficiently satisfactory.

We shall find in the inquiry on which we are about to enter much to interest and instruct; much more, indeed, than could be *à priori* anticipated, after what has been already said and written on the subject.

Its importance, we think, cannot be questioned, as it involves considerations, both of a moral and medico-legal nature, of the greatest consequence, and which it will be our object to point out as we go along.

It is a remarkable fact in the history of the subject, how very long the opinions of medical men have kept at variance on it; for although it is more than two centuries and a-half since the *corpus luteum* was first described, and nearly two centuries since Malphigi wrote (who, as we shall afterwards show, was the first author who doubted that the *corpus luteum* was connected with pregnancy,) we cannot but be struck with the circumstance, that it is still as far as ever from being settled; that medical men are still as far as ever from being at unison on the point; and that, if a case should now occur which would bring the opinion of the profession on this subject before the courts of this country, our knowledge would be found to have made very little progress since the memorable occasions when they were last brought before the public, viz. in the trial of the students at Edinburgh for disinterring the body of a lady, and that of Mr Angus of Liverpool, for the alleged murder of Miss Burns.

It is difficult to say, what can be the cause of such a stand in the prosecution of this branch of anatomical knowledge, unless it be referred, to the little attention which is paid to the appearances in the ovaries, by medical men in general. We ourselves have met with many of the best informed professional men, who had no very accurate ideas regarding the yellow bodies found in the ovaries. It is true that we have been much misled regarding the word *corpus luteum;* but it has now been properly explained; and we are convinced, that, if any one will take the trouble of comparing the bodies (be they true or false *corpora lutea*,) found in the ovaries of women, with the delineations which have been given of them by recent writers, he will have no difficulty whatever in distinguishing a true from a false *corpus luteum*.

It is matter of regret that authors of the present day should not describe more minutely the appearance of objects, found in the ovary and uterus, more especially when circumstances warrant a suspicion of these organs containing any of the products of conception.

This remark is applicable to many cases which we might mention in the works of recent authors, but more especially applies to a most interesting case which Dr Taylor of London has related, in his valuable observations on perforation of the stomach, in Guy's Hospital Reports. *

The case we do not hesitate to quote in full, as being one of painful interest.

" In June 1838, a female in a noble family, aged 23, was taken ill, and died somewhat suddenly under suspicious circumstances.

" She had been unwell for about three weeks, and was subject to occasional vomiting and disturbance of the stomach. Still her illness was so slight that it did not the least interfere with the performance of her usual duties. On a Saturday afternoon, about four o'clock, and therefore about four hours after her last meal, (although this could not be ascertained with certainty), she was taken suddenly ill, suffering from the most excruciating pain in the abdomen and violent vomiting. The skin was cold and clammy; there were other signs indicative of a mortal collapse, and the abdomen was tender and painful. It being suspected that she had taken poison, magnesia, and afterwards sulphate of magnesia were exhibited. No poison was found in the room, and she strongly denied the imputation. As there were some grounds for believing that she had been the subject of an improper intrigue with a male person in the establishment, and symptoms resembling irritant poisoning had occurred suddenly, and without apparent cause, while she was in tolerable health, it is not a matter of surprise that such a suspicion should have arisen. In spite of treatment, the symptoms continued to become worse; the vomiting was violent; and she died the following morning, as nearly as could be ascertained, about fifteen hours after her first seizure.

" Mr Hilton inspected the body, and found all the organs healthy, except in the abdomen. There were strong marks of peritoneal inflammation; the intestines were loosely adherent to each other, and a quantity of lymph was effused upon their surfaces. The cavity contained about a pint of liquid, which was collected, and reserved for analysis. This had evidently escaped from an aperture in the stomach, which was empty. The uterus was enlarged; its internal surface vascular; but presenting no appearance of embryo. The ovaries were vascular, and on one of them was seen a recent *corpus luteum*. The organs were in such a state as to lead to a strong suspicion that intercourse had recently taken place."

---

* Guy's Hospital Reports, No. viii. April 1839. Page 52.

Upon investigating the case minutely, it was proved that the patient died from perforation of the stomach from disease, and not from poison.

In reading this melancholy history, every one must be struck with its suspicious character, and equally with the satisfactory way in which its particulars have been so well explained. The conclusions, however, which the author has drawn regarding the appearance presented by the uterine organs, do not appear at all warranted by the facts recorded. "The organs were in such a state," says he "as to lead to a strong suspicion that intercourse had recently taken place." We are inclined to maintain the purity of character of this deceased young woman, even from the appearance which Dr Taylor has described in the uterine organs. What appearances, we would ask, take place in the uterus and ovaries, as the result of recent sexual intercourse? We are only aware of one case in which such has been described, and that is by Dr Bond of Philadelphia.

"In May 1827, I was invited by Dr Samuel Tucker to examine, *post mortem*, the body of a female who had destroyed herself with laudanum. She was apparently beween 18 and 20 years of age, well-formed, and in good health. She passed a night, or the most of it, *in coitu* with a young man, and before morning swallowed a large quantity of laudanum. Dr Tucker was called to her in the course of the morning, but so late, that all his efforts to restore her were ineffectual. The body was opened next morning, in the presence of Drs Tucker and Meigs. The viscera of the abdomen, so far as they were examined, exhibited no marks of disease, but the odour of laudanum was very strong in the stomach. I removed the organs of generation, and took them home for examination.

"The uterus was larger than I had ever before seen it when healthy and unimpregnated, and its colour indicated more vascularity. The ovaries were large, extremely vascular, and situated nearer the uterus than usual." "The Fallopian tubes were very vascular, so much so, as to give them a firmer and more fleshy appearance than usual, and instead of ending in loose, floating fimbriæ, appeared to terminate by an union with the ovaries, and to be very tortuous on account of the short distance between the ovaries and the origin of the tubes. On the surface of the ovaries were seen a few small vesicles of the size of shot, projecting little or none beyond the surface of the gland, and containing a slightly turbid fluid. From the ovaries and the fimbriæ, several small vesicles were seen hanging. Upon cutting open the uterus, it was found to be thickly coated with a substance having the appearance, and the strong peculiar odour of semen. Some of this substance was in the neck of the uterus. The Fallopian tubes (at least the one which was laid open) cȯntained, apparently, the same matter; but whether it possessed the seminal odour was not ascertained. Upon wiping this matter from the lining membrane of the uterus, it was found to be of a vivid red, as red as the conjunctiva in acute ophthalmia, or as if it had been injected with vermilion."

---

* Note of the *post mortem* examination of a female who committed suicide almost immediately after coitus. By H. Bond, M. D. of Philadelphia, in American Journal of Medical Science, for 1834, p. 403.

In the history of this very interesting case, it is made obvious to us that increased vascularity of the uterus and its appendages are the result of sexual intercourse. Might it not have arisen in this case from approaching menstruation? Be this as it may, however, it is obvious to us, that, if increased vascularity of the organs always took place, we should have married females, and those living as prostitutes, suffering more commonly from chronic disease of the womb and its appendages. At all events, it is certainly too much to put down increased vascularity of the ovaries and uterus, as at all indications of recent sexual intercourse. The same vascularity may arise from other causes, from menstruation begun, or about to begin, or from other sources of excitement than menstruation or copulation. We must not look upon what takes place in the lower animals, as has been shown by killing them, and examining the uterus and its appendages during heat, as well as immediately after copulation, as at all satisfactory, or illustrative of what takes place in the human female. In all the lower animals, copulation will only take place when the animal is in season; and we well know the state of vascular turgescence which exists in these organs at that time, whether connection takes place or not.

Although, therefore, the ovaries and internal surface of the uterus were in a vascular state, they cannot justify the conclusions which have been drawn from them. A case which lately occurred to a medical friend in Edinburgh, will enable us better to judge of what appearances take place, and remain for a short time the result of sexual intercourse. Even in this case, it is doubtful whether or not it might not have been the redness of menstruation about to begin.

A girl about 19 years of age, and in the employment of a milliner in Edinburgh, had been subject to attacks of pain in the epigastric region for two or three years. She had lately got into terms of intimacy with a young gentleman, with whom, it appeared, she had remained from home the whole of the night previous to her death. On the morning of her death, and previous to her returning home, she was seized with pain in the epigastrium, which became rapidly worse, extending soon over the whole abdomen, and accompanied with rapid sinking; and in spite of every remedy, she died about one o'clock of the same day. The body was examined eight hours after death. Ulceration and perforation of the duodenum were found to have occurred. The uterus and ovaries were more vascular than natural. The *os uteri* was quite red, and the internal surface of the uterus was a little more vascular than usual. The cavity of the uterus was found filled with a substance which very much resembled seminal fluid; it had its appearance and odour; and became pink-coloured on the application of heat. Both ovaries contained yellow bodies of different sizes, and some serous cysts. The yellow bodies were of an irregular form, of small size, and in fact bore all the characters of false *corpora lutea*.

We are inclined to think that a much more likely explanation of the appearances in the uterine organs, which were met with in the case recorded by Dr Taylor, is, that they were about to begin, or had actually began, the process of menstruation. All the appearances correspond with such a notion. "The internal surface of the uterus was vascular; the ovaries were vascular; and on one of them was seen a recent *corpus luteum*." We have ourselves now had a good many opportunities of examining the uterus and appendages of women dying during menstruation; and we refer to another part of this paper for an abstract of the appearances which have been noticed by other authors and ourselves during this period. Suffice it to say, that such a condition of the uterus and ovaries are the first and the last appearances which these organs exhibit during this process; that an effusion of blood of greater or less size always takes place into the ovary at this time; and that the colouring matter of this clot is absorbed; and that generally it has assumed a yellow hue by the time that the next menstrual period comes round. "On one of the ovaries of this patient was seen a recent *corpus luteum*." There is no further description of this *corpus luteum* given; but, from its having been found "on the ovary," which must mean on the surface, or very near, and including the surface, it certainly shows that it can have been of no great size, and therefore cannot have been a recent *corpus luteum,*—a recent body of this kind generally filling up the most part of an ovary. Upon the whole, then, we are inclined to look upon this body which was found on the ovary, as not a *corpus luteum,* but as one of those false yellow bodies which result from the excitement of menstruation, and that more than likely in this case it was the body which had been formed at the previous menstrual period. We fear that we have dwelt too long on this subject, and that we may have said more than is allowable in fair professional criticism; but every one must feel, and feel justly too, for the character of a young woman, blighted in consequence of appearances found after death, and appearances which we still maintain may arise in every woman from natural causes.

We had, previous to reading the above recorded case, imagined to ourselves a case of this kind, where the unfortunate individual had been the near relation of a medical man present at the autopsy. How very painful must have been that person's feelings, supposing that the case had been looked upon in the same light as Dr Taylor has done the one now recorded; and this we held out as one of the strongest grounds for every medical man making himself acquainted properly with this subject. This we now can do the more strongly, not only from the case which we have just been occupied in examining, but also from one or two which have lately occurred to ourselves. One of the cases to which I more especially refer is thus recorded by my friend, Dr Williamson, to whom it occurred. "A—— B———, aged 17 years; stout and plethoric, and enjoying excellent health, till within the last two or three months of her life, during which time she is reported to have complained of occasional slight epigastric pain. On Saturday evening, the 2d November 1839, she had been enjoying herself with some young friends. She retired to bed in the best of health and spirits about eleven o'clock on that evening, and was awoke on Sunday morning about one o'clock, with great pain over the region of the stomach and bowels, and accompanied with slight vomiting. These symptoms continued unabated till one o'clock P. M. on Sunday forenoon, when she expired, exactly twelve hours from the seizure. Upon examination after death it was found, that she had died from perforation of the stomach.* The left Fallopian tube contained purulent matter. The internal surface of the uterus was extremely vascular, but the ovaries were healthy. One of them contained numerous small and irregular-shaped yellow bodies, one of which, about the size ot a pea, was looked upon as bearing a resemblance to a true *corpus lu-*

---

* The symptoms along with the morbid appearances detailed, are here very much abridged.

teum. Its irregular form, small size, and want of the plications, so characteristic of the early *corpus luteum,* at once proved it to be a false body, and identical with the numerous other yellow bodies which existed in that ovary." It will be observed, that the appearances in the uterine organs of this case very much resemble those of the cases we previously described; and we have no doubt but that they would have been set down by many individuals, as the result of recent impregnations, to the great injury of the character of the unfortunate deceased.

We cannot conceive a harder case than the indiscriminate assertion that every yellow body found in the ovary is the result of impregnation. In such a case as we have just related, an assertion of this kind would throw a stigma upon the character of one whose character may have been previously unblemished, when death had for ever silenced her, and upon the surviving relations, a stigma; the more severe, since they have no means whatever of proving its fallacy. It is a point, then, which affects the well being of society at large, as well as the individual interests of every medical man.

And, again, the ability to distinguish a true from a false *corpus luteum,* may prove of the greatest importance to the ends of justice. It may point out, as we have attempted to do in a former part of these observations, that, in the case of a man murdering his wife, considerable provocation may have been offered to the man, from the circumstance of his wife cohabiting with another in his absence, and, as was proved on dissection, had actually conceived. Or, it may furnish the means of ascertaining, as in the case of Miss Burns of Liverpool, whether or not pregnancy, early or advanced, had existed.

While, therefore, a minute knowledge of such appearances ought to be acquired by every member of the profession, so that he may be accustomed with them, it is of consequence to know, that such is best gained by every one examining the subject for himself. The appearances in the ovaries ought to be first compared with the representations given, and then, with a previously unprejudiced mind, every one may soon distinguish them.

HISTORY OF THE CORPUS LUTEUM DURING THE SIXTEENTH CENTURY.

During no period in the annals of the history of medicine, did anatomical knowledge gain so much ground as during the sixteenth century; and the anatomy of the womb and appendages was not neglected. The structure of the *testes muliebres* or ovaries were previously misunderstood. They were believed to secrete a female seminal fluid; but nothing was known further of the anatomy of their texture, till Vesalius and others pointed out the presence of vesicles in their structure.

1561. The first author, however, who makes any mention of the *corpus luteum* is Gabriel Fallopius.* This celebrated man was appointed Professor of Anatomy at Pisa in the year 1548, and is generally believed to have died in his 40th year, in 1563. He published, in 1561, his Observationes Anatomicæ, in which he first made mention of what may be considered the *corpus luteum*. In discussing the point, whether there is seminal fluid contained in the ovaries or not, he says: "Vidi quidem in ipsis, (viz. the ovaries) quasdam veluti vesicas, aqua vel humore aqueo, *alias luteo*, alias sero limpido turgentes, sed nunquam semen vidi." It may be doubted whether or not the above passage refers to the *corpus luteum*. It certainly is the first mention that we find made of yellow bodies in the ovary, and, of course, may refer as well to false as true *corpora lutea*.

1566. Volcher Coiter† flourished about this period. He was a pupil of Fallopius, and adopted the maxims of his master. In his work, entitled "Externarum et internarum principalium humani corporis partium tabulæ atque anatomicæ exercitationes," he has left what has been considered by most authors on the history of anatomy,‡ as the first description of the *corpus luteum*. I have not had an opportunity of consulting this work, and am, therefore, sorry that I cannot quote the authority.

HISTORY OF THE CORPUS LUTEUM DURING THE SEVENTEENTH CENTURY.

It is rather remarkable, that, although a very great number of celebrated anatomists flourished during the latter part of the sixteenth and beginning of the seventeenth centuries, none of them has made any mention whatever of the subject of the *corpus luteum*, till De Graaf wrote in the year 1672. All the authors during this time, however, describe the vesicles in the ovarian tissue, and Kerkringius must have been aware of and seen the *corpus luteum*, although he nowhere describes it. Thomas Bartholinus, however, in his Anatomia Reformata, published at the Hague in the year 1660, makes the following references to the *corpus luteum*, (page 155.) "Substantia (viz. of the ovary), tota ex pluribus glandulis ac vesiculis coagmenta, raro tamen virilibus similis."

1672. In this year, then, Regnerus de Graaf wrote his justly celebrated work, "de Mulierum organis generationi inservientibus." It was the first description which any author had minutely given of the structure and general appearance of the

---

* Fallopius, Observationes Anatomicæ, apud Opera Omnia. 1584.

† Externarum et internarum principalium corporis Humani partium Tabulæ atque Anatomicæ Exercitationes. Folio, Nuremberg, 1566, and 1573.

‡ Boerhaave's Prælectiones Academicæ, Vol. vi. p. 81; and Hamilton's History of Medicine, Vol. ii.

corpus luteum during this century, and may be almost looked upon as the first distinct and proper description of the body which had been given to the public.

" Quæ vero (says De Graaf), secundum naturam aliquando tantum in mulierum testibus inveniuntur, sunt globuli qui glandularum conglomeratarum adinstar ex multis particulis a centro ad circumferentiam recti quasi ductu tendentibus conflantur, et propria membrana absolvuntur. Hos globulos non omni tempore in fœmellarum testiculis existere dicimus; quia post coitum tantum in illis deteguntur unus aut plures, prout animal ex illo congressu unum, aut plures fœtus in lucem edet. Neque illi adhuc in omnibus aut ejusdem generis animalibus, semper eodem modo sese habent. In vaccis enim flavum, in ovibus rubrum, in aliis cineritium colorem sortiuntur: praeterea aliquot post coitum diebus tenuiori substantia præditi sunt, et in suo medio limpidum liquorem membrana inclusum continent, quo una cum membrana foras propulso, exigua solum in iis capacitas superat, quae sensim ita aboletur; ut postremis gestationis mensibus ex solide tantum substantia conflari videantur; enixo jam fœtu globuli illi rursus imminuuntur, ac tandem evanescunt."*

In the passage we have just quoted, will be found a clear and distinct description of the body, incumbered by no theory, and, in short, including much in small compass. He evidently speaks from personal observation. There is one paragraph which may require a little explanation if taken alone, viz " that *corpora lutea* do not always exist in the ovaries, but only after coitus." It is evident, however, from the succeeding passage, that he refers to impregnation; one or more (*corpora lutea*) being detected in the ovary, according as the animal produces one or more fœtuses at a birth."

In the same year, viz. 1672, John Swammerdam published his work entitled Miraculum Naturæ. In this work he fully discusses the process of impregnation as it was then known. It is well known that Swammerdam was a fellow pupil of the celebrated De Graaf, at the university of Leyden, and that they both distinguished themselves as anatomists, under the celebrated Van Horne. The author's understanding of our present subject may be best drawn from his own words, " magnitudo horum ovorum (viz. the Graafian vesicles) in mulieribus non semper eadem," " sed ita ut glandularum seu vesicularum, semen prolificum continentium, non ovorum eis nomen indiderit." " Ac ne in virginibus quidem, eadem omnium magnitudo est, sed alia aliis majora sunt."

It is very evident, from the above passage, that Swammerdam* knew very little regarding the *corpus luteum*, and in fact has only casually mentioned the circumstance. We would not have thought it necessary to have made the quotation at all, had it not been that his name is so frequently quoted in connection with the subject.

---

* Regnerus de Graaf de Mulierum organis generationi inservientibus. Lugd. Batav. 1672. Pp. 297.

1675. Nicolas Hobokenus, Anatomia Secundinae Vitulinae, p. 231.

"Quam enim in sinistro testiculo aliquoties ostendebam substantiam glandulosam, aureo colore conspicuam, modo majorem, modo minorem; nunc hoc, nunc illo tenore, atque hac illam figuratione efformatam ac terminatam; in dextro invenire non dabatur."

In the above quotation there can be no doubt that our author describes *corpora lutea*, although it is probable that he might have included the false yellow bodies. We are more inclined, however, to think that he spoke of the true *corpora lutea*, and that he referred to their being seen of different sizes at various periods of pregnancy. He nowhere states his ever having found the *corpus luteum* without pregancy; on the contrary, we are inclined to think from the context, that he imagined them certainly connected with pregnancy.

1683. Steno,† the well known discoverer of the ducts leading from the salivary glands to the mouth, published this year, a description of the *corpus luteum* in his work on the muscles and glands.

"In ovario (says Steno) praeter ova quae numero plurima, magnitudine diversa, et praeter integentes ova membranas, perque membranas excurrentia sanguinea vasa et omnia connectentes varios fibrarum plexus, etiam substantia, cui incumbent, notanda, quam primo quidem intuitu pancreas salutares, is illi et color et mollities, dissectam vero liquore quodam crasso sanguineis striis, album colorem inficientibus repletum invenies; dubito tamen anne parenchyma fuerit tabefactum cum expresso hoc succo remanserit nihilominus membranae accumbens ejusdem coloris solidior, substantia multis vasis instructa, quæ nulli, quam glandulosæ magis videbatur affinis."

The above is an excellent and original description of the *corpus luteum*; for although no name is given to it, it describes so well the appearance occasionally seen in the human female, and always in the ewe and pig, that it cannot be mistaken. He does not refer at all to the use of this body, or its connection with impregnation; nor does he allude to De Graaf's description, or that given by any previous author.

1685, Peyer,‡ the equally well known discoverer of the glands of the intestines, described in this year the ovaries and their *corpora lutea.*

---

* Miraculum Naturae, sive uteri muliebris fabrica, notis in J. V. Horne, prodromum illustrata. 4to. Lugd. Batav. 1672. P. 49.

† N. Steno de Musculis et Glandulis. Lugd. Batav. 1683. p. 77.

‡ Peyerus de Ruminatione. Basiliae, 1685. P. 49.

"Profecto fœminarum testiculos a coitu affici ac immutari, palam indicant pullulantia tubercula fibrosa et glandulosa quibus ovum quodlibet fœcundum, admirando naturæ ingenio liberatur atque secernitur. Vidimus hæc et palpavimus aliquoties cum in vaccis, ovibus et capris, tum præcipue etiam in damis vulgaribus, cervini generis, quarum copiam indultu Regis Christianissimi Ludovici Magni Lutetiæ habui," "anno hujus sæculi septuagesimo octavo. Exigui quidem, fateor sunt damarum testiculi; sed post coitum fœcundum in alterutro eorum papilla sive tuberculum fibrosum semper succrescit, quo ovum seu conceptus ab ovario liberatur, ut per tubam postea descendat in uterum."

In the above quoted passage, there can be no doubt that Peyer refers to the true *corpus luteum (tubercula fibrosa et glandulosa).* There is no other body to which this description can strictly be applied.

He, *secondly*, affirms that these bodies are only to be found in one or other ovary after impregnation, he having had great opportunities of examining this subject in experiments on deer, which were afforded him through the kindness of the King.

And, *thirdly*, he looks upon the *corpus luteum* as being secreted around the ovum; which by this means, "through the wonderful ingenuity of nature," is freed from the ovaries, and then passes along the Fallopian tube to the uterus. It is important to be remembered, then, that Peyer describes the *corpus luteum* as forming around the ovum after conception, and for the purpose of pressing it outwards till it escapes from the ovary.

1686. This year is memorable in having brought to light two most excellent works, in which the subject of the *corpus luteum* is most amply and fully discussed. The first was the beautiful and original work of Marcello Malpighi, who, in a letter addressed to Sponius, makes known his views on impregnation and the *corpus luteum.* We shall quote the statements and views of Malpighi as fully as our space will permit, as they are not only new, but also are to be considered the first which look distinctly upon the *corpus luteum* as being formed anterior to, or unconnected with impregnation. His words are as follow:

"Tractu temporis luteum solidumque corpus emergit in tantam excrescens molem ut totam quasi ovarii partem papilla quadam protuberat, magnitudine completo augmento cerasi fructum aequat, (referring to the *corpus luteum* of the cow). Exterior superficies aspera est, nonnullis exiguis tumoribus inequalibus excitata, fibris carneis ambitur quae est in glandulis observatur, interius sese insinuant. Vasa quoque sanguinea

et nervi excurrunt. Membrana involvitur praecipuè in papillari appendice, quam communis ovarii tunica ulterius investit."

" In completo quoque luteo hoc corpore, interdum nil fere occurrit contenti colliquamenti, sed frequentissime ab interiori tunica papillam investiente, ubi fovea exterius utplurimum excitatur, et foramen postremo excavatur cinereum quoddam corpus producitur membraneum et fortasse fistulosum, quod versus centrum perpendiculariter elongatum in ramos vasorum instar dividitur, qui universam lutei corporis compagem excurrunt, hisque appenduntur lobuli sub diversis inclinationibus. In aliquibus adultis corporibus luteis, versus centrum et quandoque profundius, ovulum, cum appendice magnitudinis grani milii observatur.

" Sinuoso ductu pervia, qui a papillae medio foras hiat ita, ut stylum admittat : inclusa vero cavitas magnitudini pisi aequatur et ambiente membrana uno cum continuato ductu investitur. Haec itaque in diversis constitutionibus deprehensa probabiliter evincere videntur luteum hoc corpus non solum pro custodia ovuli ejusque ejectioue, a natura excitatum esse, sed fortasse ejusdem generatione conferre, hincque glandulosam potius quam musculosam censendum esse ; ejus enim structura non est fibrosa, nec carnea, sed potius succenturiatis renibus persimilis, nnde probabiliter dubitari potest, per glandulosam hoc luteum filtrum praeparari et separari materiam quae transmissa per ramos umbilicalis vasculi, tandem in ovulum seu praeviam cicatricem ichore contentam immutatur."

"Ex his quoque dubitari potest vesiculas, quae quocumque tempore in ovariis copiosae luxuriant, inequalem sortitas magnitudinem et concrescibili colliquamento plenas, non esse vere ova quae postremo foecundentur, sed materiam, ex qua fortasse glandulosum, luteumque corpus primo excitatur. Non enim videtur certum, post coitum tantum, seminisque masculini affusionem, luteam et glandulosam manifestari substantiam et hanc foecundati inclusi ovuli signa praeferre ; nam fraequentissime in vitulis nuper natis unam vel alteram vesiculam insignem deprehendi, cui lutea haec substantia graminis instar adnascebatur. In vaccis quoque praegnationis praecipue tempore, et in diversa contenti foetus aetate, lutea haec corpora interdum ciceris modo cerasorum fructum aequantia in ovariis observavi, et haec multiplicia, nec tamen superfoetationis suspicio aderat. Hoc idem in muliere, circa septimum graviditatis mensem observavi, quin et in uno eodem ovario in diversis animalibus plura lutea haec corpora unequalis magnitudinis extant, in quibus non succedit tanta foetuum multiplicitas. His addam

---

* Malpighi, Opera Omnia, p. 32-3. Epist. ad Sponio.

in pluribus ovariis concoctis praecipue, vasa ampla, luteo succo concreto turgere. Dubitari quoque potest, non unicam vesiculam albuminis absumi pro generatione lutei unius corporis sed fortasse plures insumi; nam ubi perfectum extuberat luteum hoc corpus non solum totam ferè ovarii concavitatem occupat, sed frequenter paucae albuminis vesiculae adstant cum aliis copiosissimae numerentur. His itaque pensitatis non improbabiliter colliges, luteam hanc glandulosamque substantiam non immediate subsequi seminis affusionem factam in ovo, intra ovarium contento; sed longe ipsum antecedere, et subventanea quoque dari ova, vesiculas pariter colliquamento refertas non esse rigorose ova, sed materiam, ex qua glandula excitatur, cujus ope ovum separatur, fovetur, et stato tempore cicitur."

In the above quotation there are several points which it will be necessary to examine minutely, for the purpose of observing if they really bear out the views and impressions which have been currently given to them, if the meaning of the author is precisely what is generally understood at the present day.

The first paragraph which we have quoted distinctly shows that Malpighi knew what a *corpus luteum* was, and that he was familiar with it in the cow. His description of it is also new, and not at all similar to that given by De Graaf, which we have already quoted.

He distinctly speaks, however, of two kinds of *corpora lutea*, one, which he calls the adult *corpus luteum*, " in aliquibus adultis corporibus luteis;" and another, to which he gives no name but the *corpus luteum*. He seems, however, to have applied the same term to all yellow bodies found in the ovary; for he says, " quin et in uno eodem ovario in diversis animalibus, plura lutea haec corpora inequalis magnitudinis extant, in quibus non succedit tanta foetum multiplicitas;" and " non raro luteum hoc corpus magnitudinem ciceris, pene adeptum pyrum figura mutatur: et internis a centro versus collum sensim angustatum sinum habet colliquamento turgidum,"—showing that he made no distinction in the name of these bodies, although he was perfectly aware that the one was concerned in conception, and the other not.

From these statements, it appears evident that Malpighi arranged all yellow-coloured bodies found in the ovary under the general name of *corpora lutea*. It is true that he knew and has described the true *corpus luteum*, which he says, in the cow, is of the size of a cherry; but he also applies the term *corpus luteum* to those yellow bodies, numerous in the ovaries, and about the bigness of a small vetch, " *magnitudinem ciceris*," which professional men now understand better, and refer to their true source, effusions of blood into the tissue of the ovary.

We have, *secondly*, to remark that Malpighi and the anatomists of his day laboured under great disadvantages in not knowing the true appearance of the ovum. Thus we find Malpighi, and it is probably the cause of the particular doctrines which he adopted, considering a small firm body which he found on one or two occasions in the interior of a *corpus luteum*, as the ovum. We have already shown that Peyer described the *corpus luteum* as formed around the ovum. Malpighi, however, goes a step farther, and describes the *corpus luteum* as secreting the ovum, and all this from his having once or twice observed a small firm body in the interior of a cavity within the *corpus luteum*.*

On this circumstance alone, we think, there can be little doubt that he built his particular theory regarding the body in question,—a theory which we shall afterwards have occasion to show was, *verbatim*, copied and promulgated as original by Sir Everard Home.

It appears to us, that, from the examination of Malpighi's writings, the following impartial conclusions may be arrived at.

First, That he considered all yellow substances found in the ovaries as *corpora lutea*.

Second, That he considered all those yellow bodies as formed to secrete ova.

Third, That he considered it as not proved that yellow bodies are formed in the ovary after conception.

Fourth, That the vesicles appeared to him the materials out of which the *corpora lutea* are formed, by means of which the ova are secreted, nourished, and at their proper time expelled from the ovary.

1680–1686. John Bohn, usually Latinized Bohnius, well-known as the author of a medico-legal treatise on the comparative mortality of wounds, was professor of anatomy at Leipsig. His observations on the *corpus luteum* are to be found in his Circulus Anatomico-Physiologicus, published at Leipsig in the before-mentioned year. Bohnius was a pupil, and great admirer of the doctrines of Malpighi, to whom he has dedicated his work. He differs, however, in many points from Malpighi, and we shall, therefore quote, as fully as our space will permit, his remarks on this subject. "Causa hujus subsidentiae corpus illud glandulosum est, quod fecundatam intra testem stipat ovulum et proventum suum portione hujus fluidi intrinseci debet." "Precarium hoc videbitur illis, quibus adhuc incertum, post coitum demum foecundum, glandulosam hanc substantiam manifestari idque ideò, quod in nuper natis foemellis, ac aliquamdiu impregnatis etiam observetur, item, multiplex reperiatur in ovariis illorum, animalium, quae haud multipara.

---

* We have already explained and delineated (Part 1. of these observations) cases which must have been similar to the ones above-mentioned. The minute firm body found in the interior of a cavity in the *corpus luteum* is the remains of the central fibrinous mass, which, either not entirely filling up the cavity at first, or afterwards becoming detached from the membrane lining the interior of the cavity of the *corpus luteum*, assumes the appearance which has been described by Malphigi, Home, and others.

Verum quemadmodum aliorum, imprimis, quorum fides optima et observandi dexteritas conspicua, autopsiam in dubium vocare nefas duco, ita quin oculis propriis, cum attentione in materiam, quam mente volvo, directis, diffidam, nemo mihi vitio vertet, adeoque quantum quidem his in diversis, tam humani, quam brutorum, subjectis usurpavi, nunquam nisi a coitu, me excrescentiam illam glandulosam observasse, testor. Pauci elapsi sunt menses, cum puellae sedècim, et paulo post alîus, tredecim, annorum, his ipsis, vero infanticidæ juvenculae, nona à puerperio septimana extispicium celebrarem, in quarum neutra, ne vestigium ejus, deprehendere potui. Pariter, quos uteros vaccinos et ovillos hujus rei experiundae causa hactenus lustravi, horum non impregnatorum testiculos glomis his glandulosis vacuos, foetorum vero illius tantum lateris, cujus cornu foetum gerebat, testiculum uno, effœto tamen, instructum vidi, hinc ordinariè non nisi coitus proliferic productum, substantiam hanc auguror. Sicut autem experientiâ innotescit, per petulcam nonnullarum lasciviam et nefandas frictiones ac titillationes interdum et sine omni maris cohabitatione aliquod ovulum ad uterum provocari, subventaneum equidem; ita nec ratio deest, quin in ejusmodi subjectis sibi relictis aliquando similis substantia generetur, quatenus vel a stimulis venereis intrinsecis, vel extraordinario sanguinis et succi genitalis orgasmo, exagitationem intestinam et secessum portionis lymphaticae unius alteriusve ovuli, impregnationis motui similem, contingere non impossibile est."*

The passage which we have just quoted from the works of the learned Bohn, contains all that he has said regarding the *corpus luteum*; and we connot but be struck with the following facts, as drawn from a perusal of the passage quoted.

First, That he has given no positive definition of what a *corpus luteum* is, and that, while he talks of it as a glandular body, he seems to adopt the opinion of Malpighi, that it secretes the ovule.

Second, That in no portion of his observations does he refer positively to his having seen *corpora lutea* in the ovaries of infants and unimpregnated females, as well as several in the ovaries of those pregnant with one fœtus. In no part of his observations, we say, does he mention having seen them. He only speaks and refers to the observations of others. "Precarium hoc videbetur, illis quibus adhuc incertum," &c.

Third, That his explanation of the reason why *corpora lutea* are formed without conception, viz. during lasciviousness, from titillation without connection, or from connection without impregnation, rests upon the following foundation,—that experience teaches us that it is not impossible.

---

* D. J. Bohnii Circulus Anatomico-Physiologicus. Progymnasma i. p. 18. Lipsiæ, 1686, 4to.

Valisneri, in his work referred to below,[*] states his opinion regarding the *corpus luteum.* He was a professor at Padua, and a great admirer of the doctrines of Malpighi, whose statements regarding the *corpus luteum*, he maintains, viz. that they are formed in the ovaries of virgins.

Bertrandi soon afterwards undertook the investigation of the subject. He is quoted as having proved that *corpora lutea* are formed without impregnation. His experiment was as follows. He took three female guinea pigs from their mother, and kept them separate from the male animal fifteen months, when he killed them, and in every one of these animals were found *corpora lutea* in a progressive state.[†] It appears to us that this experiment is far from being satisfactory. In the first place, it is an extremely difficult thing to tell the sexes of young guinea pigs ; and, in the second place, true *corpora lutea* are only found in a state of advancement; they were not *corpora lutea,* but only in a state of advancement. We think it likely, that, had they been seen now, they would have been looked upon as false bodies.

1689. In this year Ruysch[‡] published that part of the descriptive account of his museum relative to the female organs of generation. He was professor at Leyden, and is well known to be the person who introduced into especial notice the beautiful method of making anatomical preparations by injection. In the work we have just quoted, he nowhere describes the *corpus luteum*, but is doubtless familiar with De Graaf's description, for he cavils with him regarding the use of the word cicatrix, which he thinks ought to have been called *vestigia.* He seems to have had erroneous notions of the process by which the ovule escapes from the ovary. He talks, however, of the calyx of it remaining in the ovary, and forming the *corpus luteum.* He has delineated in his first volume a pregnant uterus, the ovaries of which are represented opened, and in one of them a *corpus luteum* is to be seen.

1695. Stephen Blancard,[†] who wrote in this year, gives a distinct and lucid account of the changes which take place around the ovum from impregnation to its expulsion from the ovary. He says, " Brevi namque post coitum, ovorum tunicas (quae ante diaphanae erant,) opacas devenire conspicimus, multisque sanguinis vasis adornari, et postero die post opacitatem, illam conspectam inter dictas ejus tunicas glandulosam quandam materiam totum ovum involventem, globulique figuram representantem intueberis."

---

[*] Valisneri, Part ii. chap. 10, delle ovagi delle Animali vivipare.

[†] Quotation from Bertrandi in Dr Seymour's Illustrations of Disease of the Ovaries.

[‡] Ruysoh, Opera Omnia, Vol. i. decas tertia, p. 15.

[§] Stephen Blancard, Anatomia Reformata. Lugd. Batav. 1695. p. 566.

It may be observed of this author, that he is the first who minutely narrates the changes which take place around the ovum before its expulsion from the ovary. He is also the first who represents the *corpus luteum* as effused between the membranes of the Graafian vesicle. From the passage we have quoted, as well as from the context, it is obvious that Blancard believed the *corpus luteum* to be the result of impregnation.

HISTORY OF THE CORPUS LUTEUM DURING THE EIGHTEENTH CENTURY.

1707. John Van Horne's,[*] Opuscula Anatomica et Chirurgica were published in 1707, and one of his tracts seems to have been the first work in which the distinction between false and true *corpora lutea* was drawn. He says, "Qualis glandulas duplicis merito generis asseveram, licebit alias quæ semper adsunt et alias quæ post coitum tantum prolificum occurrunt. Priorem quippe necessitatem, *i. e.* substantia testiculorum fibrosam aliquid glandulosi intertextum esse ex lympha ovulorum contenta per glandulas omnino a sanguine secernenda. Posteriores autem dubitantem, an post coitum solum foecundum substantia ista glandulosa manifestetur vindicat, circa usum tamen earum a *Graafio* dissentiens quod lympham ovuli genitalem minus activam sequestrare potius, quam ovulum protrudere videantur, ovulis impregnatis magnitudine potissimum non impregnatis cedentibus.

From the passage we have quoted above, it will be distinctly observed, that Van Horne draws a distinction between true and false *corpora lutea.* He is the first author who does so, and unequivocally regards true ones to be the result of conception.

1726. In the works of Philip Verheyen, the following paragraph, referring to the *corpus luteum*, is found :—

"In præsenti vero notandum singula ova insidi cuidam sinui, sicuti glans insidet suæ capsulæ ; ad quem sinum plurimæ ac tenuissimæ terminantur venæ et arteriæ ; quarum indubie hac materiam pro nutritione et accretione ovi afferunt, illæ superfluam revehunt."[†]

Verheyen flourished much posterior to the time of De Graaf, and, therefore, the statement which is to be found in the work of Santorini is erroneous, viz. that Verheyen described the ovaries, with *corpora lutea*, before De Graaf. This, however, the dates will prove, cannot be correct. Besides, De Graaf died at the early age of 43 ; and shortly after he had published his work on Generation ; whereas Verheyen's Anatomy did not appear till nearly eighty years afterwards. We conceive his description of the *corpus luteum*, which we have quoted above, is very imperfect, and evidently borrowed from the other writers of his time.

---

[*] Opuscula Anatomica et Chirurgica, 12mo. Leips. 1707.

[†] Anatomia Corporis Humani. By Philip Verheyen. Brussels, 1726.

1727. In this year, Drelincurtius[*] published his work on medicine, in which he fully discusses the doctrine of a female seminal fluid, and, amongst other questions, puts the following. Whether the maturity of the ovum, and the period of its egress from the ovary, depends upon its age and softness, or from the dryness of the peduncle which nourishes it, or from various conditions common to its investment, and especially the fleshy fibrous nests of the ova ? The author seems to propose these questions for information. It is evident, however, that he does not adopt the opinion of Malpighi, that the *corpora lutea* secrete the ovules, but he is anxious to determine if the *corpus luteum* has the power of expelling the ovum from the ovary. We have not thought it necessary to quote the words of Drelincurtius, as he nowhere expresses his opinion regarding the *corpus luteum*, or even describes it. It is obviously, however, the *corpus luteum* which he refers to in saying the " fibrous fleshy nests of the ova."

1739. The next author that it behoves us to mention is Santorini. He seems to have adopted the opinion of Malpighi, and, indeed, to follow him very closely in every respect. His observations on this subject will be found in his anatomical observations.

He says, " Quod circa lutea corpora observaverim præstat præponere." * * " Nos ea tamen in intemeratis virginibus plurimis sæpe commonstrata luculenter, vidimus ; atque adeo neque ex virili initu, tum primum excitari, neque ad maturitatem perduci, sed in iisdem conclusum ovulum solummodo fœcundari dicendum est. Eadem autem, tùm in praegnantium cadaverum tum puerperarum anatome et dilutioris coloris, et disparis magnitudinis comperimus ; quod non a variis corporis habitu, seu a duplicis fœtus germine, verum a peculiari cujusque constitùtione, prout paucis deinceps attingam, pendere puto."[†]

The above is the substance of his observations on this subject, and their close resemblance to the opinions of Malpighi will be at once discovered. We have not quoted his description of the *corpus luteum*, as it is nearly verbatim the same as that of De Graaf, which we have previously given. But it is worthy of observation, that although he has given the description of the *corpus luteum* from De Graaf, we must remember that the body which De Graaf refers to and describes was the *corpus luteum* as found in the ovaries immediately after impregnation, or during pregnancy. Our author, however, nowhere describes the *corpora lutea* of virgins, but merely mentions that they are similar to those which he has seen in pregnant women. Now, we must remember, that false yellow bodies exist in the ovaries of married women as well as in virgins; and it is not unlikely, that, in the state of knowledge at that time, he looked upon every yellow body found in the ovary as a *corpus luteum*. But it may be remarked also, that Santorini adopted the opinion first propounded by Malpighi and his followers, viz. that the *corpus luteum* was formed previous to impregnation, for the purpose of secreting the ovum. It was natural, therefore, for such believers to search for yellow bodies in the ovaries of virgins, which, if they had the colour, whatever their appearance or size might be, were put down as incipient *corpora lutea*. This certainly is a result which we cannot help coming to, both from reading the works of these authors, and from our own observations, in the present state of our knowledge, on this subject. It is certainly true, that our author quotes nearly verbatim the description of the *corpus luteum* by De Graaf; he nowhere describes the *corpora lutea* of virgins as he calls them; but contents himself with giving general statements as to the result of his observations, without descending into the slightest particulars regarding a subject which he admits to be disputed.

---

[*] Drelincurtii Opuscula Medica. Hagae, 1727. P. 711.

[†] Santorini, Observationes Anatomicæ. Lugd. Batav. 1739. Cap. xi. §. xv. Pp. 223.

There can be no doubt also, that he adopted a theory regarding the formation of the ovum which has been proved to be wrong, —a theory for the promulgation of which it was necessary that yellow bodies should be found in the ovaries of virgins; and it is not, therefore, too much to suppose, that general conclusions drawn from personal observations, founded on wrong theory, are certainly far from being satisfactory.

1748. This year, the celebrated Buffon,* gave to the world his supposed discovery of the seminal fluid in female viviparous animals, and of the reservoir which contains it. "Dans les jeunes femelles avant l'age de la puberté, la surface des ovaires ou plutot des testicules, est lisse et égale; lorsqu' elles arrivent à l'age de puberté, il se forme à la surface de ces testicules, une ou plusieurs petites protuberances, qui grandissent peu à peu, et qui croissent à une grandeur considerable. Malpighi et Valisneri ont donnè le nom de *corps jaune* et de *corps* glanduleux a ces proeminences; comme ils en ont aussi donné la description. Je ne la répéterai point ici. Je dirai seulement que ces deux anatomistes, apres avoir demontré que les vesicules de l'ovaire ne sont pas des oeufs, ont tous deux pensé que ces corps glanduleux renfermoient l'oeuf dans leur cavite; que Malpighi a meme cru l'avoir vu une fois ou deux."

It will be observed, from the preceding passage, that Buffon, *first*, did not credit the assertions of Malphigi, that he had found an ovule within the *corpus luteum* once or twice, and that the *corpus luteum* secreted the ovule.

---

* Decouverte de la liqueur seminale dans les femelles vivipares et du reservoir qui la contient, par M. de Buffon, in Memoires de l'Academie Royale des Sciences, for the year 1748.

Second, He believed, that he had found within the *corpus luteum*, a female seminal fluid.

Third, He took it for granted, that *corpora lutea* were formed in virgins, (without ever examining the subject for himself,) from the fact, that he believed the *corpus luteum* secreted this fluid at puberty, and without any impregnation whatever. The doctrine of the female seminal fluid has now assumed its proper position, and with it the opinion of Buffon, on our present subject.

1754. Kuhleman,* who wrote immediately preceding Haller, who assisted Haller with many of his experiments, and on whom Haller places the utmost confidence, is the next author whose observations refer to our present subject.

The inquiries which the author prosecutes, and the division of his subject, will be best understood in his own words.

" *Primo*, Illa (observationes) erunt enarranda, quae in virginibus, oestrum venereum nondum passis, observantur.

" *Secundo*, In illis occurrentia, quae tempore oestro venereo, jugulatae sunt, atque rem cum castratis habuerunt, coitu vero foecundo fruendi occasione destitutae fuerunt.

" *Tertio*, Ea, quae circa initia conceptionis animadvertuntur in illis, quae congressum foecundum passae sunt.

" *Quarto*, Illa, quae post decimum septimum diem in gravidis observavimus, atque magis ad progressionem fetus spectant."

Of course, with regard to our present inquiry, it will only be necessary for us to examine the first three heads of the above division.

First, Then, with regard to the young sheep in season. Their ovaries were found full, and the vesicles prominent, but no spot or fluid body of any kind was to be found in them.

Second. Of those sheep which had connection with wedders.

1st Obs. Sheep, 4½ years old. Killed eighteen hours after connection. Right ovary contained two " corpora quasi glandulosa ;" left one three.

2d Obs. Sheep, 3½ years old. Killed twenty-two hours *post coitum*. Right ovary, one old *corpus luteum* ; left ovary contained one *corpus luteum*, considered by Kuhleman to be most probably that of preceding year.

3d Obs. Sheep, 2½ years old. Killed a short while, (time not especially stated,) after copulation. Right ovary contained a *corpus luteum*, considered by Kuhleman to be doubtless that of the preceding year. Left ovary contained one enlarged Graafian vesicle.

Third. Of his third division, it may be generally remarked, that *corpora lutea* were always found corresponding in number to that of the fœtus *in utero.* Kuhleman has been always quoted as the author who has proved that *corpora lutea* can be produced in sheep while in season from connection with wedders; and although this author's observations are not very generally known, yet they are well worthy of consideration and discussion, as they present to us a most original and scientific way of settling the subject. Kuhleman found that no *corpus luteum* was to be seen in the ovaries of virgin animals killed during season; and also that the number of *corpora lutea* in the ovaries of animals killed during pregnancy corresponded precisely with the number of fœtuses *in utero.* Now, what conclusion ought to be drawn from his observations on the connection of sheep with wedders? We should distinctly answer, so far as we understand them aright, that no proper conclusion can be drawn, as in one instance bodies similar to *corpora lutea* were found; and in the other two instances, nothing at all indicative of any recent change, the result of the copulation with the wedder, had taken place.

---

* Observationes quaedam circa negotium generationis in ovibus factae, 2d edit. Lipsiæ, 1784, Auctore Joanne Christophoro Kuhleman.

As this was a curious inquiry, and one on which we had sacrificed much time and thought, it appeared of the greatest consequence that this question should be fairly settled; and with this view we set about preparations for repeating the experiments of Kuhleman. Upon inquiry, however, regarding the procuring of sheep and wedders for the intended experiments, we were astonished to find upon asking the farmers, herds, and butchers, that the circumstance of a wedder having connection with a sheep is almost unknown, and, therefore, by no means in our power could we get animals to suit the experiments which were proposed. It was rather annoying, however, to have a series of experiments so completely rendered abortive as those, and I therefore without delay applied to a particular friend, an extensive sheep-farmer in the country, from whom I received the following intelligence.

"In the course of my acquaintance with sheep breeding, which has now been extended over nearly forty years, I have never known a wedder have connection with a sheep in season, unless that wedder had not been properly cut (emasculated.) It occasionally happens in cutting these animals that a small portion of one testicle is left, or that only one is found in the scrotum, the other not having come down. Such animals will have abundant connection with sheep, and are equally useful as rams, for the females bear young afterwards; but I never knew a properly cut wedder have connection with a sheep. Indeed, it is a practice amongst us to allow the wedders to go in the parks with the sheep during the time that they are in season; and upon inquiring at one of my men who has lived upon my farm for half-a century, and in the midst of parks filled with sheep and wedders during the breeding season, he says, that he never in his life saw a properly cut wedder have connection with a sheep."

From the observations of this gentleman, on whom I am inclined to place the greatest reliance, I with reluctance gave up all idea of prosecuting my intended set of experiments. Upon referring to Kuhleman I find that he makes no mention whatever regarding the wedders. He does not even mention whether the same wedders served the sheep on which he experimented, or whether they were different ones. This is extremely unfortunate, as it is impossible for us but to be in doubt as to whether the animal used in his first experiment was an imperfectly castrated ram or not; circumstances lead us to suspect so, and the more from a remark which is made by Haller, viz. that it is difficult to procure wedders who will copulate,—three only out of a large number being found. If this is correct, which, however, is certainly not contained in Kuhleman's observations, it would lead us still more to suspect that the wedders on which Kuhleman experimented were imperfectly castrated rams, and that, had his first sheep been allowed to live till the proper time, it is more than probable that she would have proved pregnant.

We look in vain in Kuhleman's concluding remarks for his opinion regarding the *corpus luteum.* He describes most minutely and beautifully in the following words the process which takes place in an impregnated Graafian vesicle, and from which we may almost draw the conclusion that he deems the *corpus luteum* as only arising after impregnation. His words are: "post coitum ovulum Graafianum intumescere, parietes ejus carnosos fieri, generataque vera ova, et ad exclusionem jam matura, parietes corporis lutei expandi, et tandem apertura facta, ovum erumpere," "adhuc ulterius post ruptionem, in cavitate atque in margine imprimis, progerminant papillulae carneae rubrae, cruentae, et quasi acini inter se cohærentes, quae ex dictis flocculis nasci videntur."

1758–1767. In 1758 Haller,* one of the most accurate observers that ever cultivated the science of physiology, communicated to the Royal Society of Sciences at Gottingen his experimental researches on the formation of the chick and the formation of the fœtus in quadrupeds; both of which were republished in 1767 in his Opera Minora, in which are contained all his experiments upon impregnation and the formation of the *corpus luteum.* We have repeatedly had occasion to quote his observations in a former part of this paper, on the ovaries of the animals which he sacrificed for his experiments. He describes not only the appearance of the ovary and of the *corpus luteum,*† but also other bodies which he occasionally found. We cannot but expect, then, that his observations regarding the appearances in the human female should be of extreme minuteness and accuracy. He had not many opportunities of examining this subject in the human female, but we find in one of his cases the following description. "In altero ovario sinistro nempe, cicatricula caeruleo colore, pellucens in media convexitate adfuit. Ad ejus ductum, quum firmam et densam ovarii membranam removissemus, unum adparuit *luteum corpus* corni vel olivae simile, flavum, acinosum, renis succenturiati facie, nisi quod ruberet in altero extremo, fere ad mediam partem sinu quodam divisum, rotundo cæco qui flatu non elevaretur."* Such a clear description can leave no doubt as to the nature of the body he describes; and on all occasions he details with sometimes a tedious minuteness, the facts themselves from which he draws his inferences; and the following are the conclusions which his long experience enabled him to draw from them.

---

* Haller's Opera Minora, Vol. ii.

† He described the *corpus luteum* beginning to form around the ovarian vesicle, for the purpose of expelling the ovule, 22 hours after conception, having sacrificed many animals in investigating the changes which take place in the vesicle and around it, from half-an hour after fruitful intercourse till a late period of utero-gestation.

"Nullus unquam conceptus est absque corpore luteo.

"Corpus luteum in virgineis animalibus nullum est. Ex conceptione oritur neque prius partum adest." He afterwards makes the following general observation. "Sed puellae nondum corpus luteum habent, quando flos aetatis eas ad amorem invitat. Quotquot, foeminæ nullam foecundationem ante mortem passae sunt, tot etiam incisae, nulla corpora lutea ostendunt." And again, "Centum et ultra corpora mulierum aperui. Decies forte, corpora lutea vidi; neque unquam nisi in gravidis, in puerperis, aut a puerperio defunctis foeminis; neque puto frequentes esse incisores, qui decies in humano corpore corpus luteum viderint." (Opera Minora, Tome iii. p. 185, 186. Lausannae, 1768.)

It must be remembered that Haller's conclusions are always most legitimately drawn from the facts adduced by him, and placed before his readers, and that he never speaks so decidedly upon any point without the most ample and satisfactory evidence. We must remember, also, that Haller was acquainted with the works and doctrines of Malpighi and his successors; he often, indeed, quotes them in his work. The following passage, indeed, would seem to show that he looked upon Malpighi as having included more than the true *corpus luteum*, in his description of that body:—"Fibras carneas (quæ Malpighi est sententia) corporis lutei in neutro corpore vidimus. Maximi vero momenti videtur, corpus luteum non ante conceptionem in ovario fuisse sed fœcundae veneri supervenire." At all events, whether or not this may be supposed to show that Haller looked upon Malpighi to have included more than the true *corpus luteum* under that name, he distinctly states, that they are not to be found in the ovary before conception, but only after that event.

Taking the most unprejudiced view of the observations of Haller which is possible, we cannot but be struck with the philosophic way in which he went about his experiments and drew his conclusions. The candid manner in which he has minutely detailed all these experiments, by means of which others may draw their own conclusions; and the sincere and unbiassed spirit which seems to have constantly guided him throughout. He quotes those authors who are so directly opposed to him, and gives their opinions due consideration, and even when the powers of his body were breaking up, we find him writing to Buffon on this interesting subject, and embodying his convictions in the strong and eloquent terms, which we have already quoted. It would have been well for science had Malpighi and his followers described minutely the appearances they observed, and then drawn their conclusions, instead of merely stating their general opinions. We should have found less division upon this subject at the present day, had such a course been pursued.

---

* Opera Minora, Tom. ii. Pars. i. p. 47. Lausannae, 1767.

In Spence's System of Midwifery, published in 1784, at Edinburgh, we find the following paragraph. "We observe upon the surface of the ovaria, papillæ, and upon accurately examining these, we commonly discover a small aperture, and when we cut into the ovaria, where these papillæ are, we find certain bodies, which, from their colour, have got the name of *corpora lutea*, in each of which there is a small hollow, corresponding with the before-mentioned aperture. These *corpora lutea* are most remarkable in pregnant women, and those lately delivered." To this, Dr Spence has attached a plate, giving six illustrations of *corpora lutea*; and nothing can point out to us better how little this subject was understood at that time, even by those who taught midwifery, than the plate we refer to. There is not the slightest resemblance to a true *corpus luteum* in any figure of it, and, in fact, it is obvious that the author had fallen into the prevalent error, and put down every yellow body which he found in the ovary as a *corpus luteum.*

1781. Wrisberg published during this year* an excellent paper on the *corpus luteum.* It appears that he had made himself master of the subject by numerous examinations in the lower animals, and also in the human female.

He decidedly states his opinion that no *corpus luteum* is to be found in the ovaries, "ante congressum fœcundum et inde pendentem conceptionem." We have not space to copy his observations at greater length, but may state, that his description of the *corpus luteum* is precise, and can refer to no other but the true body.

1789. This year appeared the writings of John Frederick Blumenbach, referring to impregnation and the *corpus luteum.* It will be observed, from the under-mentioned title,† that the observations of the present author were written for the purpose of showing the similarity of functions between oviparous and viviparous animals; and in the course of his observations, he includes the subject of the *corpus luteum*. He argues in favour of *corpora lutea* being formed in all instances where ovarian vesicles burst,—from what is noticed in oviparous animals. Birds, for instance, as is well-known, lay eggs whether they have connection with a male or not, and, consequently, the same changes take place in the ovaries whether the eggs are impregnated or not. He therefore argues, that the same must take place in the higher classes of animals. In support of this view, he adduces the authority of Buffon, Valisneri, Santorini, and Bertrandi, all of whom have noticed *corpora lutea* in the ovaries of virgins. He also quotes the opinion of Haller, whose observations he mentions with the greatest respect, and lays considerable weight upon them; after which he says,

---

* Transactions Reg. Societ. Goettingen. Fol. 1781.

† Specimen Physiologiæ Comparatæ inter animantia calidi sanguinis vivipara et ovipara, Auctore Joannae Friderico Blumenbach, Apud Commentationes Societatis Regiæ Scientiarum Gottingensis, Vol. ix. pp. 109 to 114.

"In tanta ergo sententiarum contradictione nodum, ni fallor, solvere videtur ea quam tractamus physiologia comparata;" and he concludes as follows:—"Ignoscunt manes istarum virginum, si lutea in earum ovariis corpora non absimilem agnovisse originem suspicor, utpote qui et a puellari corpore, non minus quam in turdis et merulis, eundem œstri venerei in ovarii vesiculas effectum esse reor, sitve is viri amplexus, sitve lesbio quodam lascivioso artificio excitatus. Quadrant in eam suspicionem, quas diximus circumstantiæ, sub quibus corpora lutea in innuptis observarunt auctores aetas, &c. a 14to, inde anno, hysterica quarundam affectio, &c. nunc climati quoque aliquid tribuendum decidere non audeo, annotans tantummodo quotquot mihi hactenus apud auctores occurrerunt, ejusmodi haud inficiendi casus eos non nisi in Italicis virginibus observatos fuisse."

It will be observed that the above passage is only a generalization from the observations of authors before-mentioned, and that Blumenbach, anxious to explain, if possible, the fact that these kinds of *corpora lutea* are only described by the Italian school of medicine, thinks it probable that the climate of Italy has some effect upon the young females in that country.

In no passage does Blumenbach mention having seen virgin *corpora lutea* himself. He only reasons on what Valisneri and others have said, and what takes place in birds.

1790. We find, in the Memoirs of the Academy of Turin for this year, a paper by M. Brugnoni on *corpora lutea*.

In the year 1788, M. Brugnoni had occasion to examine the body of a robust girl of 16, who, without presenting indubitable signs of virginity, preserved all the appearances of a person who had never conceived. M. Brugnoni found, however, on the anterior face of the left ovary, which was swelled and full of vesicles, a round and sufficiently large opening, into which he could easily insert a stilet. Having laid open this ovary from one extremity to the other and through the opening, he found that it led into a well-developed *corpus luteum*. It was nearly spherical, a little flattened towards its superior surface, and about the size of a filbert. In a cavity in its centre was contained a quantity of limpid and yellow serum. Its proper substance was friable and granulated, of an obscure yellow colour; and amongst the grains small apertures were observed, with minute blood-vessels contiguous to them, which ran from the centre to the circumference. This *corpus luteum* appeared covered with two tunics, its proper covering and that derived from the ovarian parenchymatous tissue.

Some time after this, M. Brugnoni had occasion to examine the body of another girl, (aged 14,) who had the hymen entire, and indeed all the signs of virginity. In the right ovary a *corpus luteum*, the size of a grey pea, was found. It had an opening into its cavity, which admitted a stilet, and contained a liquid similar to that found in the former one. Besides the above cases, M. Brugnoni had an opportunity of examining the ovary of a female six months pregnant; and he says that the *corpus luteum* presented no cavity or communication with the external surface of the ovary. He also examined the ovaries of more than twenty pregnant bitches. "Enfin," (says the translator of the paper,) "il ne reste plus de doute que les corpora lutea n'existent anterieurement à la fecondation, puisque M. Brugnoni les a trouvés dans des ovaires des ovules qu'on sait etre steriles."

It is evident from M. Brugnoni's cases, that he was not aware that yellow bodies are formed in the ovary without impregnation, and to these, (as they have been called,) false *corpora lutea*, is certainly to be referred the appearance in the ovary of the second recorded case. "It was about the size of a pea, and having a communication with the exterior of the ovary, presenting a cavity in its centre filled with yellow serum,"—all which, indeed, are characteristic of the false body. As to the first case, it appears more probable that it was a true *corpus luteum*, and a very recent one, the aperture from the exterior of the ovary being still pervious. It reminds us very much of a case recorded in the first part of these observations, where the *corpus luteum* was a true one, and quite recent.

With regard to the conclusion we have quoted, it may be remarked, that it is the only one the author could have come to, understanding, as he did, that all yellow bodies found in the ovary were *corpora lutea*, and generally believed to be the result of impregnation. If, instead of the word *corpus luteum*, we substitute that of yellow body, and keep the word *corpus luteum* to define the true body, the result of impregnation, we shall find that M. Brugnoni has precisely stated the opinion of the most recent writers on the subject.

1797. The observations of both Dr Haighton and Mr Cruikshanks* appeared in the Philosophical Transactions for this year.

"These calyces (says Mr Cruikshanks, by which he means the membranes of the Graafian vesicle), on the expulsion of the ova, enlarge and become yellow, projecting above the external surface of the ovaria, and form the *corpus luteum*, a certain mark of conception in all quadrupeds, and in women themselves, whether the embryo is visible or not."

Dr Haighton also remarks, "I may then say that no *corpus luteum* exists in virgin animals, and that, whenever they are found, they furnish incontestible proof that impregnation either does exist, or has preceded."

The above authors were familiar with the appearances of the *corpus luteum*. They sacrificed many animals to make themselves acquainted with the appearances of the virgin ovary, and also with the appearance of the impregnated ovary at different periods. They have also given a minute and accurate account of the *corpus luteum*, and the result of their experiments and observations we have already expressed in their own words. It may be remarked, however, of Dr Haighton's experiments, which seem to leave doubt on the minds of some of the accuracy of his conclusions, that he made many experiments on rabbits by tying the Fallopian tubes at certain intervals after copulation, and he found that, although no ova were discovered in that side of the uterus on which the tubes had been tied, *corpora lutea* were found in the ovary of that side. This circumstance, however, obviously does not at all invalidate the conclusions at which Dr Haighton has arrived: as prolific sexual intercourse had already taken place before the tubes were tied, and the impregnated ova in the ovary had, of course, followed the natural changes, as had also the formation of the *corpus luteum* gone on.

1799. Burns's Anatomy of the Gravid Uterus, 1799, p. 79.

"After each pregnancy we may perceive in one or other of the ovaries, a yellowish spot called *corpus luteum*.

"By making a section of the ovaries in such a direction as to take in the *corpus luteum*, we find it to be a round body, of a yellowish colour externally, but white in the centre. This centre in the early months is hollow, and contains a fluid. It may likewise be easily filled with size, by fixing a pipe in the spermatic artery.

The correctness of this statement, regarding the possibility of filling the cavity of the *corpus luteum* with size, by injection through the spermatic artery, has not been confirmed by any other observer.

---

* Philosophical Transactions for 1797, p. 200.

HISTORY OF THE CORPUS LUTEUM DURING THE NINETEENTH CENTURY.

Baron Cuvier's observations on this subject are as follow:—

"Le nombre des vesicules a paru généralement beaucoup moins considerable dans les ovaires des animaux disséqués pendant la gestation; on y remarque à cette époque un ou plusieurs corps jaunes, dont le nombre égale toujours celui des foetus et qui occupent la place des vesicules qui se sont vidées pour la conception. Ces corps qui ne semblent d'abord qu'un epaississement des parois des vesicules, grossessent à mesure que la gestation avance, et prennent quelquefois le volume d'un cerise."*

In this passage, Baron Cuvier distinctly avers that the *corpus luteum* is alone produced by conception; that it is found to correspond to the number of foetuses found *in utero*; and that it arises from a thickening of the walls of the vesicle. Baron Cuvier does not say that he had made any original experiments on this subject; in fact, the tenor of the passage would lead us rather to believe that the sentiments which we have just quoted were the result of his inquiries into the subject, assisted, of course, by the observations which he must have had abundant opportunities of making, and which his original mind and acute powers of observation must render any opinion of his of the greatest consequence.

1819. M. Murat, in his article *Ovaire*,† in the Dictionary of Medical Sciences, gives a very good account of this subject. Without espousing any opinion of his own, he seems decidedly to lean to that one which was promulgated by De Graaf and Haller in the following words: "cependant l'autorité de Haller a prevalu, et son opinion est generalement adoptée par les physiologistes modernes."

1823. We now approach the period when Sir Everard Home discussed, and gave to the world his observations on impregnation, and *corpora lutea.*

We shall quote successively the different passages where his opinions on this subject are brought out.

"The *corpus luteum*, from its appearance, seems to be entirely a new formation." "In its increase, it compresses the parts surrounding it so much, that when of its full size, and even when there are several in the same ovary, that body is not much increased beyond its natural dimensions."

"The *corpus luteum* is of an irregular oval form, with a central cavity; its structure is a mass of thin convolutions, bearing a greater resemblance to those of the brain than of any other organ." "*Corpora lutea* are found to make their appearance immediately after puberty, and continue to succeed each other as the ova are expelled, till the period arrives when breeding no longer goes on."

---

* Leçons d'Anatomie comparée de G. Cuvier, Tome v. p. 59. Paris, 1805.

† Dictionnaire des Sciences Medicales Art. Ovaire, p. 7. Paris, 1819.

"As the object of these observations is to prove that *corpora lutea* are the glands formed purposely for the production of ova; that they exist previous to, and are unconnected with sexual intercourse, and when they have fulfilled their office of forming ova, are afterwards.removed by absorption, equally, whether the ova are impregnated or not."

"That *corpora lutea* are formed in a state of virginity is proved both in the human species and hog tribe." In the human species by an ovum arrived at its full size being found within the *corpus luteum* of a virgin with a perfect hymen, and in the hog, by an instance where Sir Everard found the ovaries of a sow six months old filled with *corpora lutea* completely formed, and their central cavities filled with blood.

Although the authority of Sir E. Home is not recognized by the present age as first-rate or very authentic, and although every scientific man must regret that the illustration of Hunter's collection fell into his hands, yet, we cannot with justice to the literature of the subject now under review, pass over his name and remarks without giving them due consideration. Sir E. Home affords us an example of a man desirous of gaining fame and reputation for himself from the labours of other men;—of a man who grossly misstated the meaning of those whose language he nearly *verbatim* quoted, and without the slightest reference either to their opinions or their printed works; works embodying as they do every thing which Sir Everard Home has given out as original, 200 years fully before Sir Everard wrote. On the failings of this celebrated man, however, it would now be unbecoming to dwell. When we inquire into the truth of his statements, we find that they are both contradictory and unreasonable.

It is true that we have now come to a proper understanding of the appearance of the true ovum, and that since the time that Sir Everard Home wrote; but this only affords us the means of estimating more accurately the value of the alleged discoveries and original observations of Sir E. Home,—discoveries which have now been proved to be delusions, and original observations, which were all made two centuries previous.

First, then, our author's definition of a *corpus luteum* is, that it is a body of an irregular oval form, with a central cavity, its structure being that of convolutions like the brain. No one can read this passage in the present state of our knowledge without being struck with the meagerness of the description, and with the fact, that it includes every yellow body which is found within the ovary; and when we take into consideration the fact, that he represents these bodies to be found at all periods of life, between puberty and old age; and further, that we now know that vesicles burst and give rise to false yellow bodies at each menstrual period;—we must be satisfied that he refers to these bodies. He may have included the true *corpus luteum* amongst the number; but that he most frequently referred to the false ones the following passage will prove. "When there are several (*corpora lutea*) in the same ovary, that body is not much increased beyond its natural dimensions." One true *corpus luteum* nearly doubles the natural size of the ovary, and when there are several it is always three or four times its usual size.

Secondly, The vaunted discovery of Sir Everard's, of the *corpus luteum* being the gland which secretes the ovum, will be found in our previous history to have been described and insisted upon by nearly a dozen of authors after Malpighi's time, he being the first who adopted that opinion, in the year 1686.

To these authorities Sir E. Home makes no reference whatever, although he evidently must have been acquainted with them, either through their own writings or through those of others. Valisneri mentioned his having found within the *corpus luteum* a body which he took for an ovum. Sir Everard describes minutely the same thing, and talks a great deal about the importance of his discovery. We have already explained (Part I. of these observations,) and delineated what both these authors had seen and described as an ovum, viz. a piece of lymph in the interior of the cavity of the *corpus luteum*. We now know well the appearance of the true ovule, and know equally well that they could not have found it. We can only remark with regard to the cases in illustration, that they are far from being satisfactory; that a hymen is no test of a woman's not being pregnant; and, indeed, it appears to us that his description of the appearances in the virgin sow resembles precisely that of the rupture of ovarian vesicles and subsequent filling of their cavities with blood, such as takes place during heat.

Sir E. Home's descriptions and delineations very seldom correspond. The whole subject, in fact, is treated in a most unsatisfactory way.

1824. In this year Dr Blundell published a series of physiological researches* on impregnation, to which, however, he has attached no decided opinion drawn from them regarding our present inquiry. We find, however, that in his more recent work,[*] he sums up the evidence of his former experiments and reasonings up to that date, in the following words. "After all that I have said, we now come down to this simple conclusion, that *corpora lutea* of fabiform shape, and large, or larger than a pea, are alone deserving of confidence as the indications of impregnation, to which may be added, that the force of this testimony will be strengthened, provided a superficial and wrinkled cicatrix be observed on the ovary above the yellow mass." Whether, however, the *corpus luteum* with all its prescribed conditions above laid down, may form in the human ovary without intercourse altogether, or even without such intercourse as may produce impregnation, I am not prepared peremptorily to decide. I prefer the cautious manner of the Academics, to the decisive manner of the Dogmatists; and shall therefore content myself in the conclusion, with expressing my persuasion merely, that the fabiform *corpus luteum*, with asteriscal cavity—of yellow colour—large as a pea, or larger, and seated beneath a cicatrix formed on the corresponding surface of the ovary, may be looked on in the present state of our knowledge, as a strong presumptive proof of impregnation; adding, however, at the same time, that I conceive a jury ought to be cautious of giving too much weight even to this evidence, when human life is at stake."

---

* Blundell's Physiological Researches, 8vo. London, 1824.

1827. The work of Baer[†] referred to below produced a complete revolution in the department of embryology. He first discovered the true ovule floating in the fluid of the ovarian vesicles of the mammalia, and on prosecuting his experiments on the subject, he had an opportunity of witnessing the formation and developement of the *corpus luteum*.

"Corpus luteum (says he) statim post ovuli ejectionem adest," "de corporis lutei genesi satis dissentiunt observatores. Me judice minime corpus novum est, sed stratum internum thecæ magis evolutum."

We have already stated our opinion regarding the question in the last paragraph of the above quotation, and we need not lengthen this communication by further quotations of the minute anatomical appearances, and anomalous structures which Baer has noticed in the human female and lower animals. He nowhere states his opinion regarding the question of the connection of the *corpus luteum* with pregnancy, and we must, therefore, refer those more particular in this inquiry to his own learned work.

1830. Dr Seymour, in his work on the Disease of the Ovaries, published in 1830, gave a digest of what had been written on this subject up to that year. His conclusions are as follow.

---

[*] Observations on the Diseases of Women. Sect xli. On the ovarian signs of impregnation, pp. 209–210.

[†] De Ovi Mammalium et Hominis genesi. Auctore Carolo Ernest a Baer. Lips., 1827. 4to. P. 20.

"That *corpora lutea* are the result of the change which takes place in the ovarium by the bursting and discharge of the ovum, occurring rarely in virgin animals, because the bursting of the ovum is not a frequent, but only possible occurrence, but always following impregnation, and diminishing as gestation proceeds."[*]

Dr Seymour, when he wrote the above passage, cannot possibly have been aware that one or more ovarian vesicles burst at each menstrual period in the human female, and at each season of heat in the lower animals, giving rise to the escape of an ovule from the ovarian vesicle, and yet no true *corpus luteum* is formed. He must have looked also upon every yellow body found in the ovarian tissue as a *corpus luteum;* he has, indeed, given a delineation to that effect. We cannot, therefore, look upon his conclusions as legitimate, according to the present state of our knowledge; and refer to another part of this communication for an explanation of the reason why different appearances are presented by the ovary after the escape of the ovule from impregnation, and the escape of the ovule during heat and menstruation. (See page 432.)

1835. In this year, the excellent article in the Cyclopedia of Practical Medicine, on the signs of Pregnancy, by Dr Montgomery of Dublin, appeared. It was afterwards published in 1837 in an enlarged form. His observations on this subject are extremely good and decisive; indeed, it may be said, that he was the first author who drew the attention of the profession, in recent times, to the distinguishing marks between true and false *corpora lutea.*

"I may be allowed to add the result of my own observations," says Dr Montgomery, "which have been now continued through a period of nearly ten years, during which time I never omitted a single opportunity within my reach, for examining the bodies of women of all ages, and under all the varying circumstances of virginity, after intercourse, during gestation, and subsequent to delivery, at different periods from conception; these opportunities having been afforded by more than one large hospital, as well as in private practice. I have also dissected hundreds of the inferior animals, with reference to this point, and have in my museum preparations of ovaries exhibiting the *corpus luteum* in different conditions in the human female, and also in cows, mares, sheep, sows, goats, bitches, cats, hares, rabbits; and my firm conviction is of the truth of both Haller's propositions, viz. 'that conception never happens without the production of a *corpus luteum,*' and that the *corpus luteum* is never found in virgin animals, but is the effect of impregnation."

---

[*] Illustrations of Diseases of the Ovary. By E. Seymour, M. D. Lond. 1830.

We need not offer any comment on the above excellent and plain observations of Dr Montgomery. They are obviously the result of long-continued experience and careful observation, and, indeed, speak abundantly for themselves.

1836. Professor Flourens's observations on generation appeared in this year, and we find his opinion of the *corpus luteum* as follows:

" La surface externe des ovaries devient rugeuse et ridée aussitôt que les oeufs commencent à apparaitre, et les rides ou cicatricules sont d'autant plus multipliées que la femme a produit une plus grande quantité d'oeufs abortifs ou fecondés. Ces petites cicatrices sont les vestiges des corps jaunes, dont nous expliquerons plus tard la mode de formation. Autant d'ovules detachés chez les animaux multipares, autant il y a corps jaunes, dont la durée est tres variable de meme que chez la femme." " Il nous est souvent arrivé de detérminer le nombre des petits renfermés dans les cornes utérines en comptant les nombre des corps jaunes." " L'excitation imprimée par la fécundation ne concourt pas, cependant, seule à favoriser le détachment de l'oeuf; car *les observateurs* citent des exemples de cicatrices bien evidentes sur les ovaries de jeunes filles vierges; de sorte que le nombre des corps jaunes ou des cicatrices ne sauroit etre un témoin fidèle de la fecondation."*

The quotation which we have just made includes the principal part of what M. Flourens says regarding the *corpus luteum*, and it may be obviously divided into those opinions resulting from his personal observations, and those which he draws from authors.

M. Flourens, as appears to us, has mistaken the generally received idea, (at least in this country,) regarding the cicatrices in the surface of the ovaries. " These little cicatrices," says he, " are the remains of *corpora lutea*," being the more numerous the more ova the female has produced. These cicatrices are generally looked upon as occupying the spot where the rupture of the surface took place to allow of the escape of the ovule. This, of course, takes place whether the ovule is impregnated or not, and consequently, therefore, is not to be looked upon as connected at all with conception or with the *corpus luteum*. We have elsewhere shown (see page 432) that these are to be abundantly seen on the ovaries of virgins, occurring in consequence of the rupture of an ovarian vesicle at the period of menstruation; and this accounts for the numbers of these bodies which are to be abundantly seen in the ovaries of indubitable virgins. M. Flourens distinctly avers that he has found that the number of *corpora lutea* always correspond to the number of ova found in the uterus.

M. Flourens nowhere states that he has seen examples of *corpora*

---

* Cours sur la Generation l'Ovologie et l'Embryologie, par M. le Professeur Flourens. Paris, 1836.

lutea formed without impregnation, he only says that other observers (referring to Malpighi, Valisneri, Bertrandi, Home, &c.) have seen them in young virgins. This we have already explained when noticing the observations of these authors.

1838. The present professor of anatomy at Aberdeen has published his opinion regarding the *corpus luteum* in the article "Generation," in the Cyclopedia of Anatomy and Physiology, p. 480. In this elaborate article, Dr Thomson remarks, "In what has now been said regarding the *corpus luteum*, that body has been described as it is found in the place of a vesicle which has been burst after fruitful sexual union; but we may remark that the same series of changes always follows the rupture of an ovarian vesicle, from whatever cause that may have proceeded. It is now well known that in some animals the rupture of ovarian vesicles and subsequent changes take place without sexual union, merely from the state of heat or venereal excitement of any kind, while in others these phenomena are never observed but as accompaniments of conception. The sow and mare belong to the first of these classes of animals. The rabbit, bitch, ewe, and cow, may be mentioned as examples of the second, as also is generally the case in the human female; but in women, as in some other females, various circumstances induce us to believe that the rupture of ovarian vesicles and the formation of *corpora lutea*, in their place, occasionally happen without sexual union, from all those causes which excite greatly the sexual organs; and we are not, therefore, inclined to admit the presence of a *corpus luteum*, taken alone, as a certain sign of sexual union having occurred; though conjoined with other signs, the presence of one or more *corpora lutea*, or the appearance of ruptured vesicles, must be regarded as good presumptive evidence."

"While the *corpus luteum* is always to be found in the ovary of a pregnant quadruped, the formation of this body is to be regarded as the uniform consequence of the rupture of the ovarian vesicles, whether that rupture shall have been occasioned merely by excitement of the organs, or by productive or unproductive sexual union; but it is only when conception and pregnancy occur that the *corpus luteum* attains its full size, and runs through the whole of that series of changes which we have described as peculiar to that body."

In the quotation which has just been made from the excellent paper of Dr Thomson, we cannot but be struck with the course which the author has adopted for the purpose of reconciling a series of facts of a dissimilar kind, and even, we might say, of a contradictory nature. Every one who is acquainted either personally with Professor Allen Thomson, or with the writings of that gentleman, must highly respect his acute powers of observation, as well as the judgment and caution with which he draws his conclusions

Dr Thomson has remarked, *first*, that a *corpus luteum*, meaning of course a true body, always results from the rupture of an ovarian vesicle, from whatever cause that may have proceeded. We have pointed out in a subsequent part of this paper (see p. 432) the conclusions which we are bound to arrive at from recent discoveries on this subject, and will not, therefore, say more at present, than that similar results do not always arise from the rupture of an ovarian vesicle,—that a very different change takes place in the ovary when a vesicle is ruptured from the result of impregnation, and from other causes.

Dr Thomson next states that vesicles burst from simple sexual excitement or unproductive sexual union.

This also appears to us a statement, the correctness of which may be questioned. We are aware that several authors, Boivin and Duges, Seymour, and others, make mention that they believe such an event occurs; but this we must distinctly deny, until it is proved by observation. No general statement will convince us; we must have observation, and that too minute, and of a kind that will bear the test of inquiry. We must therefore plead, that, until we have more positive observations on this subject, this statement of Dr Thomson's must be held *sub judice.*

Dr Thomson seems to have overlooked the fact, that yellow bodies, commonly called false *corpora lutea*, frequently are formed in the ovary, and that they have been confounded by almost all early authors with *corpora lutea.* It is unfortunate that Dr Thomson has overlooked this fact, as, in his wish to reconcile the very different observations of different observers, he has not taken into account the circumstance, that they do not all refer to one body, but to substances bearing very different appearances, and having very different origins.

RECAPITULATION OF HISTORY.

I shall now shortly recapitulate the facts which have been adduced, concerning the history of the *corpus luteum.* We have stated that Fallopius first described yellow bodies in the ovary in 1561, Volcher Coiter then described the *corpus luteum* of the cow; but it is to De Graaf, who wrote in 1672, that we owe an excellent and comprehensive description of this body. Peyer, who follows De Graaf very closely, (1685,) made, in our opinion, a great step in the knowledge of this body. He not only describes the *corpus luteum* accurately, but states that it forms around the ovum alone after conception, and for the purpose of pressing it outwards till it escapes from the ovary,—a theory which, it will be observed, is generally believed in our own day. Malpighi followed next. He was the first author who doubted that *corpora lutea* are alone connected with impregnation; and he conceived that the *corpus luteum* was formed to secrete the ovum. It will be remembered that this doctrine was adopted by many subsequent authors, and latterly given forth to the world by Sir E. Home as original. Stephen Blancard, in 1695, next describes the substance of the *corpus luteum* as effused between the membranes of the Graafian vesicle—another opinion which generally prevails at the present day. Van Horne, in 1707, paved the way for a more precise understanding of this subject. He draws the distinction between the true and false *corpora lutea*. From 1707 till 1824, all the authors who discussed this subject, either took the view of De Graaf and Haller, or that of Malpighi, every one adducing some new but unimportant fact for the advancement of his own particular doctrine. In 1827, we became acquainted with the true nature of the ovum, unimpregnated and impregnated, in the ovary and out of it, by the researches of Baer. Since then the labours of successive authors on embryology, and of Dr Montgomery of Dublin and others, on the appearances within the ovaries, have led to a thorough understanding regarding this question, which has been so long a mystery. There is one point, however, which we must not omit to mention, and which has certainly done much to benefit this inquiry, viz. Dr Lee's researches on the changes which take place in the ovaries during menstruation. They are most valuable, and have thrown much light on our present inquiry. We shall discuss them more fully afterwards, (see page 432.)

Before terminating this short recapitulation, we shall present before our readers, the names of the different authors who have written on the subject of the *corpus luteum*, and their opinions regarding the *corpus luteum*.

Among those authors who consider the *corpus luteum* as found unconnected with impregnation, may be placed Malpighi, Bohn, Verheyen, Santorini, Valisneri, Bertrandi, Buffon, Home, Seymour, Thomson.

Among the authors who consider the *corpus luteum* as alone the result of impregnation, are placed De Graaf, Hobokenus, Stenc, Blancard, Van Horne, Kuhleman, Haller, Wrisberg, Haighton, Cruikshank, Burns, Cuvier, Murat, Baer, Valentin, Burdach,* Montgomery.

From the above list, it appears that nearly double the number of authors adopt the opinion that the *corpus luteum* is alone connected with impregnation, than those who believe otherwise. We have attempted to explain on what grounds these authors founded their opinions, and if they have palpably erred, where the error has been committed. We at the same time, however, have sufficiently quoted their opinions in their own words, so that those who may not concur with our remarks and explanations, can judge for themselves from the original descriptions.

---

* We have not quoted the observations of Burdach; for although, from the whole tenor of them, it is obvious that he believes in its connection with pregnancy, he nowhere positively states it.

It may still be justly remarked, however, that while we have decidedly drawn the distinction between true and false *corpora lutea*, and shown that, on the confounding of these, much of the misconception of authors has depended, and that, further, most of these false bodies arise during menstruation in the human being, and heat in the lower animals, we say it may still be remarked, why, if an ovarian vesicle bursts during menstruation, heat, and conception, why does the same structure not result therefrom? Before explaining this question it will be necessary to premise a few remarks on menstruation, heat, venereal excitement, and conception, the more especially as we have referred to this part of the paper for our remarks on the subject.

First, Menstruation and Heat.—The great similarity which exists between these functions in the different classes of animals in which they occur has been previously explained; but it may be necessary to say, that they are the indications of puberty in both classes of animals; that they continue to occur during the whole of fœtiparal period of life; that they cease when old age arrives or the procreative faculty ceases; that they are absent as a general rule during pregnancy and in nursing; that they both cease from extirpation or destructive disease of the ovaries; that similar appearances are observed in the ovaries at these periods; and that the rupture of one or more ovarian vesicles always takes place,* followed by the production of an irregular-looking body of different size and colour in different animals, and even of different size in the same species of animal. The only difference is that, while conception occurs in the one during heat, the other does not occur during that of menstruation, but shortly after it. (Haller, Burdach, Thomson, &c.) The explanation which we previously offered of this circumstance seems still the most plausible, viz. that, while in the human being, there is an abundant discharge from the Fallopian tubes and uterus, in the other animals there is none ; the passage, therefore, of the seminal fluid being facilitated in the latter (from the extreme activity of these parts and the peristaltic action of the cornua of the uterus,) and thoroughly prevented in the former by the menstrual fluid.

---

* The rupture of a Graafian vesicle and a subsequent effusion of blood at each monthly period, has been noticed by Dr Lee in four cases, by M. Gendrin in six, by a writer in the Medical Gazette for 1840, in several instances, by ourselves in six or seven; and all presented very similar appearances. From such a number it must be looked upon as a general rule.

In the lower animals, again, Haussman, who is generally allowed to have been a most minute observer, states, that *corpora lutea* are formed in the sow during the period of heat alone, and without any connection with the male; but they are different and much smaller than those arising from fecundation; and we ourselves have had many opportunities of seeing these irregular-shaped bodies forming and formed in the ovaries of animals at the period of heat, and without any connection whatever with the male.

Second, Venereal Excitement.—By this we mean excitement apart altogether from that of menstruation or heat. We observe many authors talking of the changes which occur in the ovaries at this time, viz. the bursting of vesicles and the formation of *corpora lutea.* (Boivin and Duges, Seymour, Thomson, &c.) But I know no reasons whatever for believing that such takes place. There is certainly no recorded case where this was noticed, and we conceive it very unlikely to occur. During a considerable space of time in which our attention has been turned to this subject, we have had many oportunities of observing the appearances of the ovary during infancy, girlhood, womanhood, and old age ; and although we have occasionally seen the true apoplexy of the ovarium, as described by Andral and others, occurring unconnected with menstruation, we never yet saw the rupture of an ovarian vesicle, or the changes which result therefrom, unless at or immediately after the period of menstruation. We have carefully looked over authorities on this point; and although we find many authors referring to such as probable, and quoting others who have similar ideas, we find no author who states positively that he has seen such.

Third, Conception.—It may be unnecessary in the present day to enter into any explanation of the generally received theory of impregnation. Suffice it to say, that authors, and more lately Dr Barry, has proved, that spermatazoa penetrate up the Fallopian tubes to the surface of the ovarium (where they have been found); that Dr Barry has pointed out certain changes resulting from impregnation (alone) in the ovulum, while still placed in the Graafian vesicle, showing distinctly that the ovulum is impregnated, while within the ovarian vesicle, and some time before it leaves the ovarium.

The changes, then, which take place in and around an ovarian vesicle, which has been the seat of conception, may be shortly stateed as follow. Increased determination of blood to the ovarium ; increased determination of blood to the tissue immediately surrounding the impregnated vesicle, and to the vesicle itself ; increased quantity of fluid in the vesicle ; certain changes in the ovule of Baer, and an effusion of lymphy matter between the coats of the ovarian vesicle. By means of all or part of these the vesicle is pressed near to the surface of the ovary, a gradual thinning takes place of its membranes and those of the vesicle, at a certain (the most prominent) point, where the rupture is destined to take place ; all which changes, taking, as they do in the lower animals, and consequently must do in the human female, a considerable space of time to be accomplished, shows the extreme care which nature takes for the evolution of that minute body which is destined to perpetuate the species. Can we wonder, then, that a certain specific body is formed and left in the ovary after conception, different from that which arises from the sudden rupture of a vesicle, and an effusion of blood into and around it, as occurs during menstruation? Does it not appear reasonable that nature will be more careful for the evolution of a body which is to play such an important part in the economy of nature, as the impregnated human ovule, than she will be with one which is destined for no important use whatever? It has been urged, and may still be urged, that birds lay eggs whether they have had connection with the male or not, and that no difference takes place in the ovarium in consequence. To this it may be answered, that the ovaries of birds and mammalia are very different, the one containing very little parenchymatous tissue, the other a great deal, and, therefore, it is likely that greater changes will take place in a parenchymatous organ from that in which there is very little.

In the passage of an impregnated ovum from a bird the only change which takes place is a puckering up of the ovarium. There is no effusion whatever into it. The same occurs where unimpregnated ova pass off. In the mammalia ovules escape at the period of menstruation and heat; but we well know that the changes which take place in the interior of and around the ruptured vesicle, are different from that which occurs when they rupture from conception. This is a demonstrated fact; and it appears most likely, that it is the want of this effusion into the interior of, and around the ovarian vesicle in the fowl, which renders a comparison with what takes place in the mammalia, improper and incorrect. It therefore appears that, while we cannot form a comparison with changes which take place in the ovaries of birds and mammalia, we have come to a satisfactory explanation of the why a different structure should result, from the effects of conception and simple excitement; and which is simply this;—that Providence has arranged the order of things so beautifully that, in the most important function of the animal economy, viz. the reproduction of the species, infinitely greater care is taken (and consequently more time is spent) in the gradual formation of that body (the *corpus luteum*), which so materially assists in the evolution of the impregnated ovulum, than in the less, but still useful and more speedy changes which occur at the period of menstruation, and heat in the lower animals.

ROMILOW'S CASE OF BICEPHALOUS MONSTER.

Fig. 1.

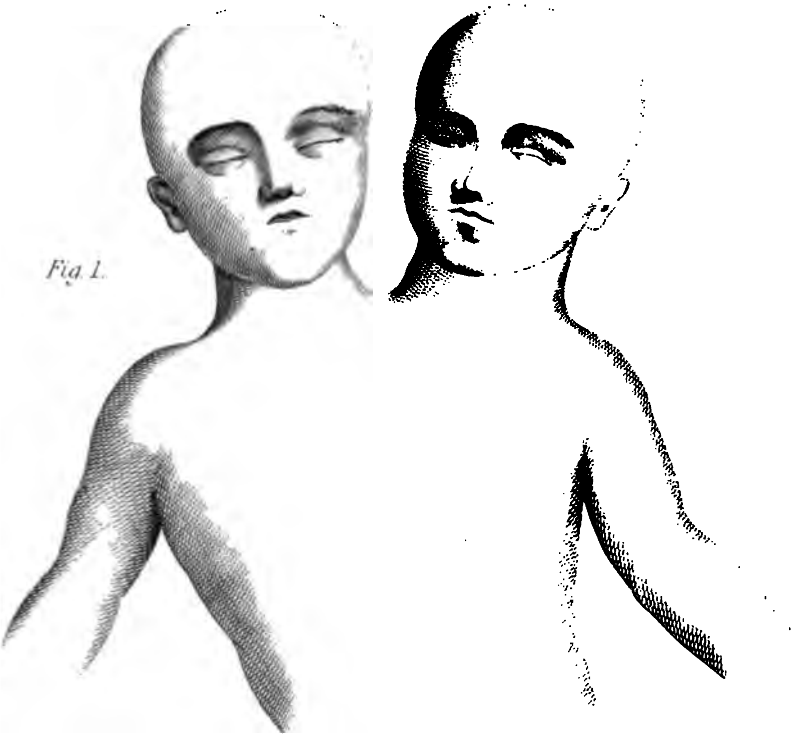
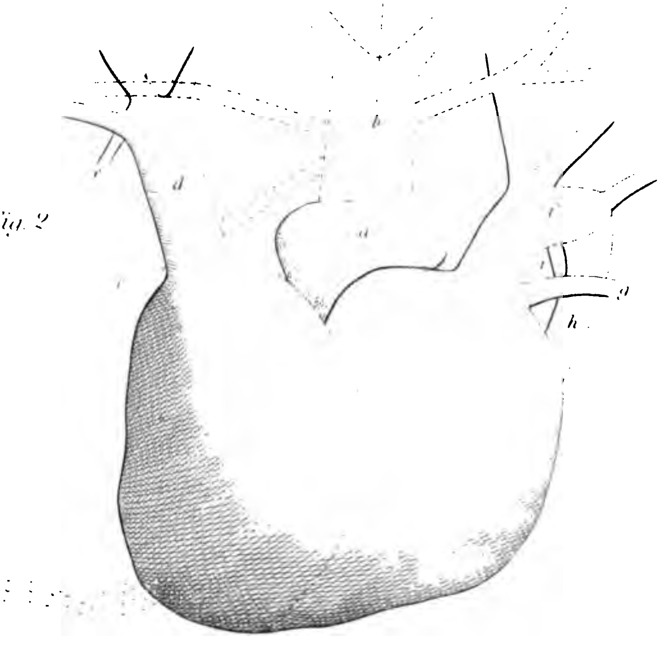


Fig. 2



Art. IX.—*Account of a Male Monster with Two Heads.* (Read and Preparation exhibited to the Medical Association, Liverpool.) By Samuel Bromilow, Esq. M. R. C. S. Lond.

On February 20th 1840, about two o'clock, I was called to attend Mary Williams, aged 20 years. She had been in labour sixteen hours. A midwife was in attendance, who informed me that the presentation had not advanced for the last eight hours. It was her first labour, and at the full period of gestation. On examination I found the head of the child advancing into the pelvis—the vertex being about the middle of the sacrum. At four o'clock the pains having abated both in strength and frequency, I gave her half a drachm of the ergot of rye, which shortly had the effect of increasing the pains, both in strength and frequency, and the head advanced so as to fully occupy the cavity of the pelvis, after which the pains again slackened. Another half drachm of the ergot was given with a similar good effect; and about nine o'clock the head was born. The body of the child not following in a few pains afterwards, I brought down the right arm, and on attempting to do the same with the left, to my astonishment I could not feel the shoulder or arm. After passing my hand along the chest of the child, I felt a large firm substance just above the pubes, the nature of which I could not clearly make out. The pains being strong, and the hollow of the sacrum not filled, I ordered the woman to be placed on her knees by the bed-side, and, satisfied that the child was dead, I pulled or rather pushed with considerable force, in the direction of the axis of the pelvis. Shortly I had the satisfaction to find that the hollow of the sacrum was fully occupied, and in about an hour the body was expelled, and immediately afterwards a second head.

The woman had a speedy recovery, being able to walk about the room in a fortnight. From the appearance of the child, I should suppose it had not been dead more than a few hours.

The heads are of the full size, well-formed, covered with the usual quantity of hair, and the bones firm. The limbs and rest of the body well-formed, and of the usual size, except the chest, being broader than an ordinary sized child. The nails on the hands and feet perfect. The genital organs well developed.

Dissection.—After removing the common integument from the fore-part of the chest and necks, the right clavicle was observed to be rather larger than the left, the latter being of the ordinary size. Between the necks there was a third or intermediate clavicle articulated at its inferior extremity to the top of the sternum, its superior extremity having no articulation, but lying loose immediately under the common integument. It gives attachment to the sterno-cleido-mastoideus and sterno-thyroideus of the one side of each neck.

The thymus gland was natural.

On opening the cavity of the chest, the heart was situated in the centre of the cavity, about one-third larger than the usual size, and has no apex, (vide Fig. 2.) On examining its interior structure, I found two cavities only: the auricle, situated at its superior part and between the two aortas, and one large ventricle occupying the whole of the substance of the heart. The orifices are guarded by the usual valves. From the left portion arises an *aorta ascendens*, which gives off the right and left carotid, then forming a curvature or arch, from which arises the left subclavian, and terminating in the *aorta descendens*, which crosses over the spines to the right side of the vertebræ. Immediately below the aorta arises the pulmonary artery, which goes to the left lungs; the *ductus arteriosus* passes nearly at right angles from it to the arch of the aorta. Another *aorta ascendens* arises from the right portion of the heart, which divides into the right and left carotid and right subclavian. About the part where the arch and descending aorta commences only a small arteryis given off, which goes to the right lungs. The *vena cava superior* is formed by the junction of the left jugular, of the right neck and right jugular of the left neck; a little lower down the right and left subclavian enter. The other jugulars enter into the subclavian veins. The *vena cava inferior* runs the usual course, terminating at the posterior part of the auricle.

The lungs in each cavity of the chest are subdivided into three lobes.

The pneumogastric nerves from each head and neck have the usual distribution to the larynxes, lungs, heart, and stomachs, except at their entrance into the thorax, the external ones passing anterior to the subclavian arteries, and posterior to the axillary veins; and the internal ones passing along the carotid and posterior to the subclavian veins: the one from the right neck going to the right lung, and the one from left to the left lung.

There are two œsophagi, two stomachs, and two duodenums, the right duodenum entering into the left about its middle; the remainder of the intestinal canal being single. The rest of the viscera were natural. From each head there is a perfect spinal chord and canal to about the third lumbar vertebra, where the canals and chords unite.

Description of Plate X.

Fig. 1. View of Double-Headed Monster.

Fig. 2. The heart of a double-headed male monster, with two heads. *a.* The auricle. *b.* The *vena cava superior.* *c.* The pulmonary vein. *d.* The right aorta, giving off a right and left carotid and subclavian, from which a small artery (*e.*) goes to the right lungs. *f.* The arch of the left aorta, giving off a right and left carotid and left subclavian. *g.* The pulmonary artery. *h.* The *aorta descendens.* *i.* The *ductus arteriosus.*

34, *Great George Street, Liverpool,*
February 1841.

ART. X.—*On the Order of succession in which the Vital Actions are arrested in Asphyxia.* By JOHN REID, M. D. F. R. C. P. E., Lecturer on the Institutes of Medicine, President of the Anatomical Society, &c.*

A knowledge of the order of succession in which the vital actions of the body are brought to a stand in asphyxia, is not only useful in elucidating the exact nature of the function of respiration, and in pointing out rules for our guidance in the direction of certain remedial agents, but it may also be brought to bear in an important manner upon the investigations into the general laws of physiology. The inquiry is one, however, of unusual difficulty, from the intimate manner in which the respiration is associated in the higher animals with the other vital functions, and the rapidity and energy of the actions and reactions of these upon each other. In conducting such experiments, it is not only necessary to watch closely every phenomenon which presents itself, however fleeting it may be, but all the varied concomitant circumstances must also be carefully analysed, and, if possible, insulated, with the view of ascertaining how far they might affect the results. Our progress in such investigations must, therefore, always necessarily be slow, frequently vacillating and uncertain. These difficulties were found so perplexing, that I had several times nearly given up the present inquiry in despair; and it was not without much labour, and repeated failures, that I arrived at what I considered satisfactory results.

The two points in the physiology of asphyxia which have of late years principally attracted attention are, the nature of the impediment to the circulation of the blood through the lungs; and the cause of the arrestment of the sensorial functions. A correct knowledge of the manner in which the vital actions are arrested in asphyxia, is supposed to be included in the true explanations of these two facts. The first of these, viz. the impediment to the free passage of the blood through the vessels of the lungs, and its consequent stagnation in the right side of the heart, and the large vessels leading to that organ, have been attributed to three causes,—the cessation of the mechanical movements of the chest; the effects of the venous blood upon the contractility of the heart; and the difficulty of transmitting the venous blood through the capillaries of the lungs, when the chemical changes which go on there between the blood and the atmospheric air have ceased. The opinion, that the blood in death from asphyxia chiefly accumulates in the right side of the heart and the large vessels leading to it, in consequence of the stoppage of the mechanical movements of the chest, was advocated by Haller.[*] He maintained, that when the lungs were distended with air, as in inspiration, that the blood flowed readily and abundantly through the pulmonary vessels; but, on the other hand, when these organs had collapsed, as in expiration, the pulmonary blood-vessels were so compressed, and their angles rendered so acute, that they became in a great measure impermeable to the blood sent from the right side of the heart.[†] [Goodwyn[‡] argued, in opposition to the mathematical calculations and reasonings adduced by Haller, that, when the lungs are diminished in their bulk, and the acuteness of the angles of the blood-vessels changed only to the extent which occurs during expiration, the flow of blood through them would not be materially obstructed. He also drew additional arguments in favour of this opinion, from the continuance of the circulation through the lungs, when an amount of fluid was present in the chest sufficient to compress the lung to the extent which occurs in expiration, whether this fluid had been effused in the human species from disease, or induced by artificial means in the lower animals.[§] Goodwyn maintained that the cessation of the circulation in asphyxia was chiefly dependent upon the venous blood failing to excite the contractions of the left side of the heart. "When respiration," he says, "is obstructed, the florid colour of the blood is gradually diminished, and the contractions of the left auricle and ventricle soon cease. The cessation of contraction arises from a defect of a stimulating quantity in the blood itself."[‖] The views of Goodwyn were attacked a few years after their promulgation by Coleman[¶] and Kite.[**] Both these authors adduced the results of various experiments, to prove that the left side of the heart can contract vigorously upon venous blood;[††] and they also both maintained that they had proved experimentally, that, when the lungs are kept mechanically distended during the process of asphyxia, that the quantity of blood found in the right side after death is not found to preponderate much, if at all, over that contained in the left side.[‡‡] Bichat also furnished abundant evidence to prove that the left side of the heart can contract vigorously upon venous blood. In numerous experiments he found, that when an animal is asphyxiated, black blood at first traverses the lungs to reach the left side of the heart, and may for a short time be projected from a cut artery, with very considerable force ; and he further satisfied himself, that the contractions of the heart could be renewed even after they had become quiescent in different kinds of violent deaths, by injecting venous blood along one of the pulmonary veins towards the left side of the heart.* Bichat especially dwelt upon the importance of discriminating between the effects of asphyxia upon the functions of animal and those of organic life ; of ascertaining the priority of the suspension of those two great sets of functions, and the influence which they exerted upon each other. He maintained that the heart's action does not cease because the dark blood transmitted to the left side of the heart cannot excite it to contract, but because the dark blood, by being circulated through the coronary arteries in the muscular tissue of the heart, arrests its contractility. This effect of the dark blood upon the contractility of the heart was, however, regarded by Bichat as only an isolated phenomenon in asphyxia; for he believed that the vitality of all the tissues of the body was equally affected by the circulation of this dark blood, and that the functions of the brain or the animal functions were always arrested before those of organic life.† He maintained that the accumulation of the blood in the right side of the heart did not depend upon any mechanical obstruction in the blood-vessels of the lungs, but from various other causes, among which he enumerates the obstacles opposed to the force of the already enfeebled right side of the heart, by the effects of the circulation of dark blood in the bronchial arteries, and the cessation of the excitation of the lungs by the atmospheric air,‡ aided by the circumstance, that the systemic ventricle can more easily overcome the resistance presented by the capillaries of the body generally, than the veins and pulmonic ventricle can overcome that arising from the capillaries of the lungs. Bichat appears to have entertained doubts whether the circulation of the venous blood through the capillaries of the systemic circulation arrested the vitality of the tissues simply by default of excitation, or by exerting some deleterious influence upon it; for, while discussing its effects upon the brain, he thus expresses himself—" Je ne puis dire si c'est négativement ou positivement que s'exerce son influence ; tout ce que je sais, c'est que les fonctions du cerveau sont suspendues par elle." Although Bichat failed in giving the correct explanation of the manner in which the vital actions are arrested in asphyxia, yet there can be no doubt that to him we are indebted for having pointed out the true path by which this knowledge was to be attained. Another important advance was made in the elucidation of asphyxia by the experiments of Dr David Williams of Liverpool and Dr J. P. Kay. Dr Williams,* in experimenting on this subject, found "that when the chest is laid open immediately after the trachea has been tied during the acme of inspiration, the pulmonary veins soon become empty, while the pulmonary artery continues full." From these experiments he inferred, that, in asphyxia, the blood is obstructed in its passage through the lungs, while its circulation through the other tissues of the body continues; and that the obstruction in the lungs "arises from a deprivation of pure atmospheric air." Dr Kay, from his numerous experiments,† has also arrived at the conclusion, that "the circulation is arrested after respiration ceases; because, from the exclusion of oxygen, and the consequent non-arterialization of the blood, the minute pulmonary vessels, which usually convey arterial blood, are then incapable of conveying venous blood, which therefore stagnates in the lungs."‡ Dr Kay believes that this stagnation of blood in the right side of the heart and pulmonary artery, occurs in consequence of venous blood being incapable of exciting the arterial capillary blood-vessels of the lungs. The experiments of Dr W. F. Edwards§ upon frogs, and those of Dr Kay upon warm-blooded animals, have very distinctly proved, that the circulation of venous blood in the muscular tissue not only does not exert any deleterious influence upon its contractility, but that this property continues to manifest itself considerably longer when venous blood is allowed to circulate through the vessels of that tissue, than when the circulation of the blood has been entirely arrested.

---

* An epitome of this paper was read at last meeting of the British Scientific Association.

---

[*] Elementa Physiologiæ, Tom. iii. Lib. viii. Sect. iv.

[†] Opus cit. Tome iii. p. 246. 1776.

[‡] The Connection of Life with Respiration. London, 1788.

[§] Opus cit. p. 40–47.

[‖] Opus cit. p. 85.

[¶] A Dissertation on Suspended Respiration, 1791.

[**] Essays and Observations, &c. on the Submersion of Animals, &c. 1795.

[††] Coleman, Opus cit. p. 118; and Kite, Opus cit. p. 26, 42, and 44.

[‡‡] Coleman, p. 107 to 116; Kite, "From these experiments, it is evident that a small quantity of blood can pass through the lungs when they are in a state of perfect expiration."—P. 58.

---

* Sur La Vie et La Mort, article sixieme, § ii.

† Car, d'après ce que nous dirons, l'affaiblissement qu' éprouve alors le cœur n'est qu'un symptôme particulier de cette maladie dans lequelle tous les autres organes sont le siége d'une semblable débilité.

‡ " Le défaut de son excitation par l'air vital."

Though the experiments of Drs Williams and Kay have demonstrated, that in asphyxia the circulation is first brought to a stand by some impediment to its free passage through the lungs, yet we believe that few will feel satisfied, after a careful analysis of them, that they enable us to determine whether this impediment results from the cessation of the respiratory movements of the chest, or from the arrestment of the usual chemical changes between the blood and the atmospheric air,—a question of considerable importance in general physiology. When we remember the great influence exerted by the respiratory muscular movements upon the force with which the blood is transmitted along its vessels, —a fact first well illustrated by Hales,‖ afterwards by Bichat,¶

---

[*] On the Cause and the Effects of an Obstruction of the Blood in the Lungs. Edinburgh Medical and Surgical Journal, Vol. xix. p. 524.

[†] The Physiology, Pathology, and Treatment of Asphyxia, 1834.

[‡] Opus cit. p. 181.

[§] De L'Influence des Agens Physiques sur la Vie, p. 9, 1824.

[‖] Statistical Essays, Vol. ii. p. 1 to 33, 1740.

[¶] Sur la Vie et la Mort, article huitième, § ii.

and latterly in a more definite manner by Magendie[*] and by Poiseuille,[†] a degree of uncertainty must always exist in interpreting phenomena observed in experiments upon asphyxia, in which means have not been taken to obtain the extent and value of this influence.

Such a precaution is the more necessary, since it has been ascertained that dark blood passes at first in the usual quantity through the lungs, and is sent with great force and in a full stream from a cut artery; that it is not until the respiratory movements have been considerably impaired that it begins to stagnate in the lungs; and that after death, considerable quantities of dark blood are frequently obtained from the left side of the heart. Coleman found that the relative quantity of blood in the two sides of the heart after drowning varied considerably; " sometimes being as 7 to 4, at other times as 5 to 2, or as 12 to 7. So that, at a medium, the proportions of the right one to the left are about $3\frac{2}{8}$ to $1\frac{6}{8}$. After hanging, the medium was found to be as $2\frac{7}{8}$ to $1\frac{4}{8}$.[‡]

Professor Alison, with the view of supplying this defect in the theory of asphyxia, performed several times the following experiment.[§] A rabbit was confined in nitrogen gas until its respiratory movements had become laboured, and insensibility was approaching The animal was then withdrawn as rapidly as possible from the glass jar in which it had been confined, and the brain was suddenly crushed by a blow with a hammer, and the chest was immediately laid open. The quantity of blood found in the right side of the heart preponderated considerably over that in the left, and, as the respiratory movements had not been interrupted until the animal had been deprived of life, and the circulation in a great measure suspended, these experiments are obviously greatly in favour of the opinion, that the accumulation of the blood in and around the right side of the heart is dependent upon the cessation of the chemical changes between the blood and atmospheric air in the lungs, and not upon the arrestment of the mechanical movements of the chest.

It appeared to me that very conclusive evidence might be obtained on this question by a series of experiments performed in the following manner. A tube with a stop-cock was fixed into an opening in the trachea, and one of Poiseuille's hemadynamometers was introduced into the femoral artery, for the purpose of obtaining definite information upon the force with which the blood was transmitted along the arterial system. The stop-cock of the tube in the trachea was then shut, and when the respiratory process had been suspended sufficiently long to cause a decided fall in the column of mercury supported by the blood sent along the femoral artery, a large bladder full of pure nitrogen with a brass nozzle provided with a stop-cock, was fixed in the tube in the trachea, which it fitted accurately, and both stop-cocks opened. After the effects of the respiration of the nitrogen gas had been ascertained, a bladder of the same size as the other, similarly provided with a nozzle, and full of atmospheric air, was then substituted for the bladder containing the nitrogen, and the results compared. The difference between the effects of the respiration of the nitrogen gas and the atmospheric air was most marked, and of such a nature as could not be mistaken; for while the mercury continued to fall in the instrument during the respiration of the nitrogen gas, it rose very rapidly immediately after the atmospheric air had entered the lungs and acted upon the blood. In this experiment the same mechanical movements of the chest which failed to renew the free circulation of the blood through the lungs when nitrogen gas was inspired, rapidly effected that object when atmospheric air was permitted to enter the lungs, even when tried on the same animal, and subsequent to the failure of the nitrogen, and, consequently, at a more advanced stage of the process of asphyxia. This experiment was repeated several times, and when the requisite care was taken to procure and employ pure nitrogen, invariably with the same results.*

---

[*] Journal de Physiologie, Tom. i. Leçons sur les Phénomenes Physiques de la Vie.

[†] Journal de Physiologie, Tom. viii. p. 272.

[‡] Opus cit. p. 18.

[§] Edinburgh Medical and Surgical Journal, Vol. xlv. p. 103.

Before directing the attention of the reader to a table containing the results of one of these experiments, it will be necessary to take notice of a very unexpected phenomenon which presented itself, and for a considerable time completely embarrassed and perplexed me. Before commencing these experiments I conceived from *a priori* reasoning, that when the blood had become dark in the arteries, and the animal functions had been suspended, that the mercury would begin to fall gradually and steadily in the hemadynamometer, and that there would in a short time be a marked depression in the level of the mercury. The mercury, however, actually stood higher in the instrument, and the large arteries became more distended and tense for about two minutes after the animal had become insensible, when the blood in an exposed and unobstructed artery was equally dark as that in the accompanying vein, and when the attempts at respiration were few and imperfect, then before the stop-cock in the trachea was shut, and when the animal was breathing atmospheric air freely. This was so unlooked for, at first sight was so inexplicable, and so much at variance with my preconceived notions on the subject, that I was strongly inclined to believe that there must be some source of fallacy; but after repeating the experiment more than twenty times, and invariably with the same results, I was at last compelled to admit its accuracy. I then began to surmise that this arose from an impediment to the passage of the venous blood through the capillaries of the systemic circulation, similar to that pointed out in the capillaries of the pulmonic circulation, by which the force of the left ventricle was principally concentrated in the arterial system, and on placing a hemadynamometer in the vein of the opposite limb, and comparing its indications with the instrument fixed in the artery, this supposition, as may be seen from the annexed tabular view of the results of one of these experiments, appeared to be verified. This fact may explain how a quantity of blood is retained in the left side of the heart in asphyxia. It was also ascertained that, though the fall of the mercury in the instrument after the animal was nearly asphyxiated took place very gradually at first, it at last fell very rapidly. Suppose, for example, that the mercury in the tube ranged between 4½ and 5 inches in height before the entrance of fresh air into the lungs was prevented, it rose above this when the animal had ceased to struggle; it afterwards fell very gradually to between 3 and 4 inches; and when it had fallen to between 2 and 3 inches, it frequently sunk very rapidly to the original level. When atmospheric air was allowed to enter the lungs after the mercury had sunk low in the instrument, no sooner had the air acted upon the blood in the lungs, than the mercury instantly sprung up several inches, and when the blood had become more perfectly arterialized, it again stood lower, and the range was more limited. The respirations were necessarily much diminished in frequency, also slow and heaving after the stop-cock was opened in an advanced period of the process of asphyxia, and it was remarked, that during each respiratory movement the contractions of the heart were not only performed with increased strength, but likewise with greatly increased frequency. When the animal was breathing freely through the tube in the trachea, was quiescent, and when the blood was fully arterialized, the range of level of the mercury in the tube seldom exceeded half-an inch, sometimes not so much. When the stop-cock was shut no change took place in the range of the mercury during the first half minute: generally before the end of the first minute the animal had begun to struggle, and then the range greatly increased,—rising during each attempt at expiration, and during the struggling of the animal, falling during each attempt at expiration and during quiescence. In some of the experiments the range of the mercury during these different conditions amounted to about nine inches, and in one experiment to ten inches,—making a most material disproportion in the extent of the pressure upon the inner surface of the arterial system of vessels.

---

* In the experiments first performed the mercury rose in the instrument, but the nitrogen was mixed with a quantity of atmospheric air, as was proved by the blood becoming partially arterialized in an exposed atery.

Table I.—Showing the changes in inches, of the height and range of the mercurial column in the vertical limb of the haemadynamometer in one of the first class of experiments, when the instrument was fixed in the artery only; the intervals of time at which each change occurred, reckoning in half-minutes from the commencement of the operation; with remarks on the state of the animal at these respective changes. The depth and height of the mercury marked at the end of each half-minute indicated, as near as possible, the extent of the range in the level of the column during that interval of time.

Intervals of Time.	Height of Mercury in the tube attached to the Artery.		Remarks on the state of the Animal.
Minutes.	Depth.	Height.	
		1.0	When the haemadynamometer was adjusted to artery, the mercury stood at this height in the vertical tube of the instrument.
		5.5	At the instant the stop-cock was turned, it was 5.5.
½	4.0	4.5	Stop-cock on trachea shut. Dog quiet.
1	3.0	7.0	Do. The artery becoming a little dark.
1½	3.0	9.0	Do. The artery black. Animal struggling.
2	2.0	12.0	Do. do. Animal struggling violently.
2½	4.0	9.0	Do. do. Animal quiet.
4½	4.0	8.0	Do. do do.
5	4.0	8.0	Stop-cock on trachea opened, and a bladder filled with nitrogen gas applied.
6	3.0	6.0	Do. do.
		11	Bladder of nitrogen removed, and one filled with atmospheric air applied.
7½	5.0	11.0	Bladder removed. Natural respiration allowed.
8½	5.0	6	Do. Animal quiet.

Table II.—Showing the same conditions in regard to the second class of experiments, in which haemadynamometers were applied to both the artery and vein at the same time.

Intervals of Time.	Height of Mercury in the tube attached To the Artery.		To the Vein.		Remarks on the state of the Animal.
Minutes.	Depth.	Height	Depth.	Height.	
		0.5		0.0	When the haemadynamometers were adjusted to the vessels, the mercury stood at these heights in the two instruments respectively.
		6.0	5.0	6.0	
½	4.0	5.0		4.0	Respiration natural. Dog quiet.

Interval of Time.	Height of Mercury in the tube attached				Remarks on the state of the Animal.
	To the Artery.		To the Vein.		
Minutes.	Depth	Height.	Depth.	Height.	
2½	3.5	5.0		4.0	Stopcock on trachea shut.
3½	3.0	6.0		3.5	Do.
4	2.0	11.0		12.	Do. animal struggling. The mercury thrown over the top of venous tube, which was 12 inches high.
4½	5.5	10.0		12.	Mercury stood at top of venous tube.
5½	5.5	9.0		8.0	Do.
6½	5.0	11.0		3.5	Do.
7½	5.0	8.		2.5	Do.
8½	2.5			2.1	Do.

In some of the other experiments, the difference between the height of mercury in the two instruments when the blood became venous was not so marked as in this.

In performing these experiments, I derived much valuable assistance from several gentlemen, but more especially from Mr James Spence and Mr K. T. Kemp.

We now proceed to examine the explanations which have been given by physiologists of the cause of the arrestment of the sensorial functions in asphyxia. We have already stated that Bichat maintained that the suspension of the sensorial functions was caused by the circulation of venous blood in the arteries of the brain; while Dr Kay believes that he has proved that it is principally dependent upon a diminished supply of that fluid being sent along the systemic arteries, in consequence of the impediment to the circulation through the lungs, and not because the blood sent to the brain is venous—an opinion somewhat similar to that maintained by John Hunter.* The experiments of Dr Kay, in which he injected, "gradually and gently,"† four drachms of venous blood into one of the four arteries conveying arterial blood to the brain, through a very small syringe, "having a beak with a capillary bore,"‡ though undoubtedly sufficient to prove the highly unsatisfactory nature of the evidence adduced by Bichat in support of his position, that the sensorial functions are arrested by the circulation of venous blood in the arteries of the brain, cannot, however, be adduced as satisfactory evidence against the doctrine itself. Such an experiment may prove that the transmission of a certain quantity of venous blood along *one* carotid artery is not sufficient to produce cerebral derangement; but it cannot enable us to determine what would be the effect of the passage of venous blood along *all the four* arteries of the brain. We have very frequently watched an exposed carotid artery in an animal during the process of asphyxia, and have observed that the blood flowing along it gradually becomes darker and darker; and we were satisfied that considerably more venous blood than in the experiments now referred to, is circulated through the brain for a short time before the animal is seized with convulsions and insensibility. It is evident, then, that, if the suspension of the sensorial functions is caused by the presence of dark blood in the arteries of the brain, it must be circulated in greater quantities, and for a longer time than occurred in these experiments of Bichat and Dr Kay. Before we can proceed further in this inquiry, it will be necessary that we examine the variations in the quantity and force with which the blood is sent along the arteries, and returned by the veins during the process of asphyxia. We have already stated that the arterial pressure, as ascertained by the hemadynamometer, is very little altered during the first half minute after the entrance of fresh air into the lungs has been suspended; that about the end of the first, or the beginning of the second minute, when the animal commences to struggle, the pressure is greatly increased; and that, generally, for about two minutes after the animal had become insensible, and had consequently ceased to struggle, the pressure was even greater than before the commencement of the experiment. It was also repeatedly ascertained, that the venous pressure, as indicated by the hemadynamometer introduced into the jugular and femoral veins, was equally great for a short time after the animal had become insensible as before the respiration had been suspended. When an artery is cut across, immediately after insensibility has supervened, the blood springs from it in a full stream, and with a force equal to what would occur if arterial blood was circulating in the vessels. The insensibility in asphyxia cannot therefore depend upon any diminution in the force with which the blood is sent along the arteries of the brain, nor upon any diminution in the vascular pressure upon that organ. As, however, the frequency of the pulsations in the arteries becomes remarkably diminished before the circulation has been fairly suspended, we are naturally led to inquire if any change in the quantity of blood sent along the arteries of the brain could account for the suspension of its functions. With this view, we performed several experiments upon dogs. A tube furnished with a stop-cock was introduced into the trachea, and firmly secured there, the femoral artery was then laid bare, that the changes in the blood might be observed and the number of pulsations more carefully reckoned. We shall give the details of four of these experiments. After the femoral artery had been laid bare, the pulse ranged from 105 to 120 in a minute, and the respirations were very short and rapid. At the end of the first half-minute after the stop-cock was turned, the pulse was 92. At 1½ minute the pulse was about 120, the animal had begun to struggle, and the blood in the artery was decidedly dark. At the second minute the blood in the artery was nearly as dark as in the accompanying vein, but, from the struggles of the animal, it was impossible to reckon the pulse. At the end of 2½ minutes the animal had ceased to struggle, was evidently insensible, and the pulse was 42. At the beginning of the fourth minute, the pulse was still 42. The stop-cock was now opened, and the animal allowed to breathe. When the blood was becoming red in the artery, the pulse was 78. A short time after this, when the animal was rapidly recovering its consciousness, the pulse was 60, and the respirations about 132. In another experiment the pulse was 80 at the time when the stop-cock was closed. At the end of the first minute the pulse was 114, and the blood was decidedly darker, and the animal was struggling. At the 1½ minute the animal was struggling, and the blood was nearly as black as in the accompanying vein. At the end of 2½ minutes the pulse was 60, irregular in frequency—two beats following each other rapidly; the animal had ceased to struggle, and the blood was as dark as in the vein. At the end of the third minute the pulse was still 60, and irregular. In a third experiment the pulse was 100 before the stop-cock was turned. At the end of one minute the blood was getting dark, the animal had begun to struggle, and the pulse was 120. During the course of the second minute it struggled violently, and the pulse could not be reckoned. At the end of 2½ minutes the animal had ceased to struggle, the respirations were few and heaving, and the pulse was 78. At the end of the third minute the pulse was 60. In a fourth experiment the pulse ranged from 88 to 96 before the stop-cock was turned. After half-a minute the pulse was 71, and the blood was somewhat darker. After 2½ minutes the animal had ceased to struggle, the blood was as dark in the artery as in the vein, and the pulse was 70. At the end of the third minute the efforts at breathing had nearly ceased, and the pulse was 66. In such experiments as these, it is impossible to ascertain the exact frequency of the pulse at the precise moment when the sensorial functions are suspended, in consequence of the struggles and convulsive movements with which this is preceded, Taking, however, all the circumstances of the experiments into account, and combining with them the facts ascertained in those previously detailed, to prove that the arterial and venous pressure is not diminished at the time that the animal has become insensible, we have little difficulty in arriving at the conclusion, that, though the pulse has become less frequent about the time that the insensibility has supervened, yet that this has not taken place to such an extent as to justify the opinion, that the arrestment of the sensorial functions depends upon any diminished transmission of blood through the vessels of the brain. If a diminution in the frequency of the pulse to the extent we have indicated could produce insensibility, this would frequently present itself during the course of disease, and under other circumstances where nothing approaching to it is observed. It must also be remembered that the pulse, as ascertained before the experiment had been commenced, must have been more frequent than usual, from the terror of the animal. In these experiments I regarded the animal as in a state of insensibility when the struggles and convulsive movements had ceased. The function of respiration continued for a short time after the suspension of the sensorial functions, but rapidly became enfeebled. The circulation of the dark blood in the vessels of the encephalon, therefore, arrests the functions of the cerebral hemispheres before those of the *medulla oblongata*.

---

* Hunter's Works by Palmer, Vol. iv. p. 168–170.

† Opus. cit. p. 194.

‡ Opus cit. p. 193.

Dr Kay has performed several experiments,* from which he has drawn conclusions very different from those which we have just stated. He found, that when the abdominal aorta was cut across in a rabbit of the ordinary size, " nearly seven drachms and three-quarters of blood would escape from the divided aorta when respiration was unobstructed." He then proceeded to cut this vessel across at different periods after the admission of fresh air into the lungs was precluded, and found that, when cut across half a minute after this, the blood collected almost equalled what would have escaped if the free access of air into the lungs had been permitted. In another animal it was cut across after a minute and a half, and five drachms of blood escaped; when postponed to two minutes and a-half, four drachms were collected; and when delayed to the termination of the third minute, only two drachms were collected. In judging of the value to be attached to these experiments of Dr Kay, two circumstances are to be taken into account—the time an animal requires to bleed to death; and the precise time at which the sensorial functions are arrested. As there can be no doubt that an impediment to the circulation through the lungs does occur in the course of the process of asphyxia, it is, therefore, a matter of considerable importance to ascertain not only the precise time at which the sensorial functions are arrested, but also the average period of time which the blood would continue to flow from a cut artery when the respiration is unobstructed, before we can venture to determine whether there is any relation between the suspension of the sensorial functions and the arrested circulation in the lungs. With the view of satisfying myself on these points, the abdominal aorta in a rabbit breathing naturally was cut across a little above its bifurcation. The blood continued to flow freely for about one minute; it flowed feebly for another minute; and very feebly for about forty seconds more. In this experiment two minutes and forty seconds elapsed before the bleeding from the artery had ceased. Though in some subsequent experiments the hemorrhage had ceased in a somewhat shorter time, yet we believe that in the rabbit it seldom stops before two minutes have elapsed. With regard to the other point we have mentioned, viz. the exact period at which the sensorial functions are arrested, this has been most unaccountably overlooked by Dr Kay. He seems not to have been aware that a dog generally becomes insensible in from two to two and a-half minutes, and a rabbit in one minute and a-half after the complete occlusion of air from the lungs, so that experiments such as those he has related, made to ascertain the quantity of blood which flows from a cut artery at periods posterior to the occurrence of the suspension of the sensorial functions, cannot be adduced in explanation of effects which have previously happened. In performing the experiments as I have already mentioned, I took the cessation of the struggles and the convulsive movements of the animal as a test of insensibility. When a ligature is tied tightly around the trachea of a rabbit, the animal moves about nimbly at first, but, before one minute and a-half have elapsed, it has fallen down in a state of insensibility, and the attempts at respiration are few and heaving. As the manifestation of the functions of the *medulla oblongata*, upon which respiration depends, are not necessarily linked with that of the functions of the cerebral hemispheres or the sensorial functions, it must be evident that, in attempting to discover the cause of the cessation of the mechanical movements of the chest, the frequency of the respirations ought to be attended to, and not the suspension of the sensorial functions. This circumstance has not been overlooked by us in performing these experiments; and we are satisfied that the function of respiration is much enfeebled at a period of the process of asphyxia, when this cannot be explained by any diminution in the quantity of blood sent to the *medulla oblongata*. No doubt respiratory movements may be observed after the pulsations have been very considerably diminished in frequency, but these have become few in number, and performed at long intervals before this condition of the circulation has been induced; but it is quite possible that the ultimate cessation of the functions of the *medulla oblongata* may be hastened by the diminished quantity of blood sent along the arteries supplying it. If we proceed, therefore, to analyse the experiments of Dr Kay, bearing in mind the length of time the blood continues to flow from the divided abdominal aorta of a rabbit, and the precise time at which the sensorial functions are arrested in the process of asphyxia, we must arrive at very different conclusions from those which he has deduced from them.

---

* Opus cit. p. 185—88.

In further confirmation of the views we are advocating, we may appeal to the experience of every practical physician; for he cannot have failed to observe the gradual torpor that frequently creeps over the sensorial functions in severe cases of bronchitis, when an ill-arterialized blood is circulating in the vessels of the brain, and the pulse is still pretty strong at the wrist.

We feel very strongly convinced that Dr Kay has fallen into another error in stating, that, three minutes after the entrance of air into the lungs had been prevented, the blood in the arteries had assumed the venous hue " still imperfectly ;" for, in numerous experiments, various gentlemen who were present all agreed that the colour of the blood in the arteries was as dark as that contained in the accompanying veins at a period anterior to this. The statement of Bichat, that the blood in the arteries exactly resembles venous blood in a minute and a-half or two minutes, is, I am satisfied, much nearer the truth.

From the various facts we have mentioned, we have arrived at the conclusion, that the suspension of the functions of the encephalon are chiefly, if not entirely, dependent upon the circulation of venous blood in the arteries. We do not, however, maintain that venous blood exerts any noxious influence upon the functions of the nervous texture; but believe that the effects are solely to be attributed to the want of the proper excitation of the organ; for, when the circulation of arterial blood is renewed, its functions rapidly remanifest themselves, provided that this be done within a given time.

We believe, then, that, in asphyxia, the order of succession in which the vital processes are arrested is as follows;—The venous blood is at first transmitted freely through the lungs, and reaches the left side of the heart, by which it is driven through all the textures of the body. As the blood becomes more venous, its circulation through the vessels of the brain deranges the sensorial functions, and rapidly suspends them, so that the individual becomes unconscious of all external impressions. The functions of the *medulla oblongata* are enfeebled about the same period that the sensorial functions are arrested, but are not fairly suspended for some time longer. Immediately after the sensorial functions are suspended, and the blood has become still more venous, it is transmitted with difficulty through the capillaries of the lungs, and consequently begins to collect in the right side of the heart. A smaller quantity of blood must now necessarily reach the left side of the heart; and this diminution of the quantity of blood sent along the arteries, conjoined with its venous character, and the ultimate arrestment of the circulation, being circumstances incompatible with the manifestation of vitality in the other tissues of the body, general death is sooner or later induced.

The persistence of the muscular contractility after the arrestment of the circulation varies, as we have had frequent opportunities of witnessing, according to the age and strength of the individual, and also in a very marked manner from constitutional causes, which are unknown; and in this way we are able to explain how the heart's action may be renewed a considerable time in some cases after apparent death, while in others all the attempts to restore animation, though commenced shortly after the suspension of the sensorial functions, have failed. It must be obvious that the first and principal object in the treatment of asphyxia is to restore the circulation through the lungs. If once we succeed in this, and thus renew the heart's action, the arterial blood is again transmitted to the encephalon and the other tissues of the body; the functions of the *medulla oblongata* remanifest themselves; the sensorial functions are gradually restored; and the animal heat returns. The derangement of the functions of the *medulla oblongata* and the sensorial functions are not necessarily coequal in extent, and never in importance, in asphyxia, and this is well observed in some of those cases of death from disease or narcotic poisons, where the process of asphyxia occurs more slowly and gradually. In these it is not unusual to find the sensorial functions nearly or entirely suspended, at a time when the respiration is pretty effectively carried on; and it is evident from various facts, that the arrestment of the muscular respiratory movements is not dependent upon the suspension of the sensorial functions, but upon those of the *medulla oblongata.*

We shall now proceed to make some remarks upon the increased force with which the blood is sent along the arteries during muscular contraction. It has been proved, as we have already mentioned, that the blood is sent with greater velocity and increased force along the arteries during the contraction of the muscles of the limbs and trunk, as in exercise, and this takes place in a more marked manner during violent attempts at expiration. On the other hand, during violent attempts at inspiration, the pulse becomes less frequent, feeble and soft. In some of the experiments we performed, as we have already mentioned, the mercury rose as high as the eleventh, and in one to the twelfth inch of the scale attached to the tube, during violent attempts at expiration, and the struggles of the animal; while it fell as low as the second inch during violent attempts at inspiration. During these different conditions, the pressure upon the external surface of the heart and its position in the chest must be somewhat altered, a certain amount of pressure being applied to its outer surface during expiration, and removed during inspiration; and it recedes deeper into the chest during inspiration, and again comes forward during expiration;* but we may safely set those aside as exerting any appreciable influence in the production of the phenomena in question. Müller believes that the increased contractions of the heart accompanying muscular movements of the trunk and limbs may be caused by a sympathetic or reflex action,—an excitant effect being produced in the filaments of the nerves distributed in the contracting muscles, which, being conveyed inwards to the spinal cord, is reflected upon the heart. As, however, he adduces no direct evidence in favour of this opinion, we do not feel inclined to abandon the old explanation, that this is merely dependent upon the mechanical acceleration of the blood, by the pressure exerted upon the blood-vessels by the surrounding muscles during their contraction, and the more especially, as we have witnessed several facts, which at least prove that a great part of the phenomena in question may arise from this cause. We have frequently remarked, that when an animal was breathing very rapidly, even above 100 in a minute, through a tube in the trachea, that the mercury did not rise higher in the instrument than before, and that the range was limited, provided the expirations were always short, and, consequently, not attended with much compression of the blood-vessels in the thorax and abdomen. On the other hand, a marked rise of the mercury took place whenever a forced expiration was made, however slowly this was performed. It was also repeatedly observed, that when one instrument was fixed in the femoral artery, and another in the femoral vein of the opposite limb, the mercury stood considerably higher in the instrument fixed in the vein than in that fixed in the artery, when the animal began to struggle violently. In few of the experiments did the mercury rise much above eleven inches in the instrument in the artery, while it frequently ran over the top of a tube twelve inches high, with considerable force, in the instrument fixed in the vein, —showing us in some of these experiments a prodigious increase in the pressure upon the inner surface of the venous system, equal to between three and four pounds on every square inch of surface. This greater elevation of the mercury in the instrument fixed in the vein, can only be explained by the effects of the mechanical pressure of the surrounding muscles becoming increased, as the extent of the vascular tubes over which it is exerted becomes elongated, and may afford some indications of the greatly increased impulse communicated to the blood by the powerful pressure exerted by the contraction of the muscles of the chest and abdomen upon their contained blood-vessels, when aided by the contractions of the muscles of the limbs, and favoured by the presence and particular disposition of the valves of those blood-vessels. It is difficult to determine, then, how much this increased flow of blood along the vessels during violent expirations, and during the contraction of the muscles of the limbs, depends upon more forcible contractions of the heart, or upon the mechanical effects of temporary pressure upon the blood-vessels. The increased rapidity and strength of the contractions of the heart during violent expirations must be partly attributed to the compression of the blood-vessels of the lungs, and the transmission of an increased quantity of blood to the left side of the heart, while the diminution in the strength and frequency of the pulse during inspiration must, in a great measure at least, depend upon the sudden removal of that pressure, so that a great part of the blood propelled during a few of the contractions of the right side of the heart, which immediately succeed the sudden dilatation of the thorax, goes to fill up the blood-vessels of the lungs to that state of plenitude in which they were before the preceding expiration, and a small quantity only reaches the left side of the heart.

---

* In mentioning this fact in the article Heart, in the Cyclopedia of Anatomy and Physiology, the word inspiration has been inadvertently printed for expiration, and *vice versa.*

We do not think it necessary to make any remarks upon the question, whether or not the blood stagnates in the lungs, in consequence of the cessation of the chemical changes between the blood and the atmospheric air, or upon any supposed effect which the venous blood may have upon the contractility of the capillary vessels of the lungs, as this has already been most ably and most satisfactorily done by Dr Alison. He has shown that this phenomenon is to be referred to an interesting general law in physiology, which has hitherto not received the attention which its importance demands, by which the movements of nutritious juices is influenced by the chemical changes, or, as he terms them, the vital attractions connected with the chemical changes, which are constantly going on in the capillary vessels between those juices and the surrounding tissues, by which nutrition and secretion are effected. That such a moving power exists, regulating the quantity of blood which flows through each individual organ, independent of any impulse from the living solids, cannot be doubted.* Before arterial blood can be transmitted freely through any tissue or organ, it is not only necessary that the contractions of the heart be performed with a certain amount of force, but that the actions of nutrition and secretion be also in operation ; so in the same manner, before the blood can be transmitted through the lungs, it is not only necessary that the right side of the heart retains its contractility, but that the chemical changes between the blood and the atmospheric air should proceed. This doctrine is still further illustrated by the fact which we have ascertained, that, when the blood in the systemic circulation becomes decidedly venous, and unfit for carrying on the process of nutrition, it passes less freely through the capillary arteries into the veins.

---

* Vide Outlines of Physiology, 3d edition, p. 22–25, 61–64, and 224.

Art. XI.—*Collection of Fluid in the substance of the right Lobe of the Thyroid Body.*

By J. Massey, Esq. Surgeon, Nottingham.

The subject of the present case is a man 30 years old, whose health has been tolerably good, having never required medical aid, except for the present disease, which he states has existed at least ten years. When first noticed by him there was a little enlargement on the right side of the windpipe, about the middle of the neck, which gradually increased, and after five years duration was about one-half its present size. At this period he first applied for relief. I then examined, and considered it an enlargement of the right lobe of the thyroid body; directed six leeches to be applied occasionally, and to rub in, night and morning, *Ung. Potass. Hyd.* and take the solution of iodine three times a day. There was no fluid perceptible at the period. He continued the use of the remedies for about three months, after which he says the swelling was softer, and somewhat reduced. It was not painful, and gave him but little inconvenience; and he allowed its increase without any further treatment till the present time.

October 30th. The tumour is situated over the right side of the thyroid cartilage, extending a little above, and for some inches below it,—the longest diameter being obliquely from before backwards, passing under the sterno-mastoid muscle externally. There is no pain on pressure, nor does he complain except from its inconvenience in size. It measures round the neck over its most prominent part eighteen inches. There is evident fluctuation, which is most apparent at its anterior and inferior surface. Here it is more prominent than elsewhere. There is also a pulsation communicated throughout its extent from its contiguity to the carotid artery, from which circumstance it has been by some considered aneurism; but this pulsation is not perceptible, after carefully removing the tumour as much as possible forward from the artery. His complexion is sallow; says he has never had jaundice, though there is now a slight bilious tinge of the conjunctiva. The bowels for some time have been very costive, motions frequently scybalous, of a brownish colour, but never like pipe-clay; urine high-coloured, but never marked his linen; his habits have been temperate; his breathing is difficult, accompanied with a wheezing sensation, much increased on exertion, particularly stooping; and he feels as if he has a ligature round his neck. There is dryness of the throat with a little cough. He has vertigo, frequent palpitation, and profuse perspiration on increased exertion.

I directed him to renew the use of the iodine treatment, which he continued till November 27th 1840. At this time there was no perceptible alteration in the tumour. I therefore introduced an exploration needle, at the point where the fluctuation was most evident, and evacuated by it about five ounces of fluid, having the appearance of very thin bile, of an olive-brown colour, with an oily crystalline deposit on its surface. This almost completely reduced it to its natural size.

December 1st. After the needle had been used, the wound immediately closed. To-day the swelling has increased to about one-half the size it was before the puncture. I determined to let it accumulate in larger quantity, and then freely to lay open the sac, and keep it discharging by pledgets of lint introduced, secured by adhesive straps.

4th. The tumour has increased to its former magnitude. I therefore made an opening into the sac, four or five inches in extent, by means of a sharp-pointed bistoury, and evacuated at least five ounces of the same kind of fluid as before. (This was the second fluid, vide analysis, in which was a quantity of blood, escaping from the incision made in the sac and adjacent parts, which was not in the first quantity taken, November 27th.) The finger introduced into the sac could be passed above the thyroid cartilage, and downwards, very nearly to the sternum, and outwards under the sterno-mastoid muscle. The thyroid body was converted into a large cyst, containing the fluid; a pledget of lint was introduced into it, secured by adhesive straps. A very small quantity of blood was afterwards taken from his arm for analysis.

5th. Wound discharged very freely during the night. On removing the dressings about four ounces of fluid, as before, escaped, of a deep-red colour, from being mixed up with a quantity of blood. Has very little pain; dressed as before.

6th. There is more swelling of the parts, which is probably from increased vascular action; discharge is less bloody, escapes very freely; bowels costive.—*Cap. Pil. Cal.* gr. v. *statim, et postea Pulv. Jalap. Co.* ʒi.

7th. Discharge is exceedingly offensive, and averages throughout the day, he says, about eight ounces in quantity.

9th. Wound daily dressed, discharges freely.

14th. The discharge is more purulent, less in quantity, still very offensive. There is more swelling of the thyroid body and adjacent parts, than immediately after the first evacuation of the fluid. Bowels very costive, it being now more than four days since they were relieved. Repeat the purgative, and take three times a day *Sulph. Quin.* three grains; *Sulph. Magnes.* half-a drachm; water one ounce and a-half; apply over the pledget of lint a linseed meal poultice.

17th. The discharge daily diminishes, is now more sero-purulent; bowels torpid, motions brown; appetite good; the breathing is much more free; the palpitations and cough are very much better; and the sensation as of a ligature round the neck is not now perceptible. *Capt. Pulv. Jalap. Co.* ʒi. *mane p. r. nata.* Continue the tonic, and have daily injected into the sac some of the following solution :

℞ *Potass. Hyd.* ℈i ; *Iodini* gr. iii. ; *aq. distillat.* ℥xvi. *M. ft. Injectio.* Afterwards apply a linseed poultice.

22d. The injection has been daily used ; discharge has considerably diminished ; there is now no bilious tinge of the conjunctiva ; bowels open ; there is very little more fulness than natural ; the neck measures at least two inches less. Continue as before.

28th. Still improving ; discharge is very trifling, more serous. His general health is much improved, and he will probably be quite well in a few days ; bowels are still torpid. Continue as before.

As we are but very imperfectly acquainted with the functions of the thyroid body, any pathological phenomena, in reference to the morbid alteration of its structure, or its abnormal secretion, will be acceptable, as a means of elucidating its actual use, and importance in the animal economy.

Fatty matter, as well as urine, and its peculiar principles, have been detected in unnatural localities ; and that other organs and tissues beside the liver may acquire the power of forming and separating cholesterine, and some of the other principles of bile, from the blood in large quantity, will be fully corroborated by the present case.

It is probable that the enlargement of this body primarily consisted in a derangement of its cellular structure, with an increased and vitiated exhalation into the cells, and their subsequent partial or general dilatation and thickening. The previous morbid exciting cause still continuing, an increased secretion into the cells with their further dilatation and rupture was the result ; so that the lobular structure of this body, in consequence of the additional accumulation, ultimately became converted into a large sac, which contained the adventitious fluid.

The accompanying analysis of the fluid and blood is made by my much esteemed and highly talented friend, Dr Wright,[*] in whose accuracy, founded on his extensive chemical knowledge, I have the utmost confidence.

First portion of fluid, removed November 27th.

Slightly alkaline, specific gravity, 1.0242

Water,	905.140
Mucus,	19.830
Albumen,	5.210
Gelatine,	11.100
Albumen, combined with soda,	8.350
Cholesterine,	10.640

---

[*] For an account of his discovery of the presence of alcohol in ripe fruits, vide Lancet, September 29, 1838 ; also his valuable analysis of the ergot of rye, and detection of its active principle, vide Vol. liii. of Edin. Med. and Surg. Journal.

Oily matter,	5.200
Colouring matter soluble in water and alcohol,	8.250
Biliary matter,	9.730
Chlorides of sodium and potassium,	6.210
Carbonates of lime and soda,	4.380
Iron,	.250
Loss,	5.710
	1000.000

Second portion of fluid, removed December 4th.

Alkaline, and containing hydrosulphuret of ammonia—specific gravity, 1.0356

Composition.

Water,	896.310
Mucus,	34.270
Albuminate of soda,	7.920
Fibrine,	1.840
Cholesterine,	9.560
Colouring matter soluble in water and alcohol,	16.340
Gelatine,	10.830
Resin, not affected by nitric or muriatic acid,	5.820
Chlorides of sodium and potassium,	7.460
Phosphates of lime and soda,	3.210
Iron a trace.	
Loss,	6.440
	1000.000

Blood taken December 4th, Specific Gravity, 1.0435

Composition.

Water,	810.520
Fibrine,	2.890
Albumen,	43.740
Colouring matter,	104.950
Crystalline fatty matter, probably cholesterine,	12,620
Oily matter,	6.350
Biliary matter,	5.820
Salts, Chlorides, Phosphates, Sulphates, soda and lime,	8.310
Loss,	4.800
	1000.000

The different results of the analyses of the fluid, taken at two different periods, viz. 27th November and December 4, may probably be accounted for, by a quantity of blood being mixed with the last taken, as well as the system being under the influence of medical treatment. The first fluid removed contained no blood.

The peculiar composition of the fluid is remarkable; for though each constituent of bile is occasionally found in other organs, as the kidney, appendages to the uterus, &c., I believe there is no case on record in which a secretion like the present has been found connected with the thyroid body. The questions in reference to the case which present themselves are;—

Whether the bilious tinge of the conjunctiva depended, wholly or in part, on absorption of the fluid from the sac? Or is it more probable that it depended entirely on the condition of the liver?

It is a remarkable fact, that, as soon as the fluid was completely evacuated, the yellow conjunctival tinge disappeared.

Did the palpitation and cough depend upon the pressure of the tumour on the pneumogastric nerve, and on the cervical branches of the sympathetic?

Nottingham, December 29, 1840.

ART. XII. — *Cases of Cerebral Disease.*—By BENJAMIN BELL, Esq. Fellow of the Royal College of Surgeons.

CASE I.—A respectable tradesman, 50 years of age, tall, rather spare, and of dark complexion, enjoyed good health, till the spring of 1838, when he suffered much from pain in the stomach, loss of appetite, and other symptoms of dyspepsia. During the ensuing summer he got completely rid of these complaints. His appetite became remarkably good; he grew perceptibly stout; and every one about him took notice of his improved appearance. He continued in this state till October, when his friends began to observe something unusual in his manner, accompanied by a marked deterioration of his memory. They remarked that he had a disinclination to meet even those with whom he was most familiar, and would sometimes cross the street to avoid them; while he was extremely apt to forget the every-day duties connected with his business, making mistakes altogether at variance with his former accuracy and precision.

I had occasion to see him several times in the earlier part of January 1839, and was struck by his unusual loquaciousness, having previously found him a man of few words, retiring and reserved. He now talked with more energy, emphasis, and excitement of manner, than the subject of our conversation seemed to demand; and, at the same time, went repeatedly over the same ground, in the course of a few minutes. I thought no more about this change in Mr R.'s demeanour, till a medical friend, who also had occasion to transact business with him, made a similar observation in the following April. He found him then exceedingly confused, and incapable of understanding, or at least executing a piece of work which he had undertaken.

Antiphlogistic remedies were now had recourse to; cupping; shaving the head, with cold applications; blisters; antimonial solution; active purging and low diet. At first the bowels appeared to be loaded, and bile deficient; but the evacuations gradually assumed a more healthy character. Under the use of these remedies, the confusion in great measure disappeared; his manner became more calm and natural, and his memory so much improved, that he could talk connectedly upon business matters; but it was not deemed prudent for him to take any active part in the management of his affairs.

In the middle of June, after having taken a greater latitude for some time in regard to diet, he had a recurrence of the foregoing symptoms, accompanied by flushing of the countenance, and heat in the head, with a temporary numbness in the fore-part of the right thigh and leg. Recourse was again had to depleting remedies, and in a few days he had returned to his former condition. As the summer and autumn advanced, his mental faculties underwent a gradual deterioration. He talked very slowly, and often confusedly; seldom attempted to read, and could make no progress whatever at his writing-desk, although his chief pleasure seemed to consist in examining its contents. Yet with all, he was quite contented and happy, apparently seldom thinking of his business, or any other source of anxiety. His countenance gradually lost its intelligence,—and nothing had the effect of giving it the least animation, unless it were an allusion to the simple articles of food which were allowed him, and which now occupied a large share of his attention.

About the middle of August, he fell into a state bordering upon coma, with flushing of the countenance; extreme drowsiness; stertorous breathing; and a full strong pulse; but recovered from it in a few days, after copious blood-letting, and other antiphlogistic remedies. He then relapsed into his former condition, little alteration taking place for nearly three months, unless that the mental faculties became gradually more and more impaired. In fact, the mind seemed to have become almost a *tabula rasa*, while the animal and vital functions still continued with their accustomed regularity. About three weeks before death, which happened in the middle of December, he became feverish, oppression in the chest supervened, and then coma, which lasted for one or two days.

Inspection.—The skull-cap was in general remarkably thin, and had an indentation of the frontal bone corresponding to the cicatrix of a wound, which had been received in early boyhood. There was a considerable quantity of effused limpid serum upon the surface of the arachnoid, and also underneath that membrane, separating it from the *pia mater*. About half an ounce flowed from the ventricles, but the *septum lucidum* was quite entire, and the other central parts of the brain seemed to possess their natural consistence.

The preceding case seems to present several points of considerable interest. 1. The remarkable disappearance of severe dyspeptic symptoms, and the improved state of general health, which preceded and ushered in, and accompanied the earlier indications of disease within the cranium. 2. The decided relief afforded by vigorous antiphlogistic measures, both at the commencement and on two later occasions; while, in spite of them all, the disease still advanced from one stage of its progress to another, till every mental faculty had fallen before it. 3. It is interesting to observe that this obliteration of the mental faculties was unaccompanied by any paralytic or spasmodic symptoms, unless we consider as such a very transient feeling of numbness in one leg, which was thought at the time to depend upon an awkward position of the limb. 4. The morbid appearances within the head, when connected with foregoingphenomena, seem to illustrate one or two points in the physiology of the brain. It will be remarked that the morbid effusion was confined almost entirely to the surface of the hemispheres, and, under these circumstances, we might reasonably expect, as was the case, that the cortical substance—that is to say, the ganglionic portion of the hemispheres, would be the part to have its function chiefly interfered with. In ordinary cases of hydrocephalus, on the other hand, where convulsions and paralysis are much more prominent symptoms than disturbance of the mental faculties, we find a larger effusion, and more decided appearances of inflammation in the ventricles or other central textures of the encephalon.

This case may be contrasted with the following, in which the effusion presented similar characters, but was probably poured out with much greater rapidity.

Case II.—I was requested about midnight, July 19th 1839, by my friend, Dr Patison, to accompany him to the house of a woman in Greenside, who was suspected to have taken laudanum. She appeared to be above 50 years of age. We found her seated in an arm-chair, her head thrown back; the countenance and lips pale; the mouth open; the eyes half-concealed by the upper lids, and the pupils dilated; the respirations at long intervals and stertorous; the pulse imperceptible. She expired before we had time to employ the stomach-pump or any other remedy. There was no smell of laudanum either about her person, or in the apartment, which was small, and crowded by friends and neighbours.

The accounts which we received were not very clear or satisfactory; but it appeared that she had been seen that evening about six o'clock in her ordinary state of health by one of the neighbours, who, being sent for two hours afterwards, found her insensible, and seated very much in the same posture as when she died. Another woman said that the deceased told her she had taken laudanum; but, upon looking at the phial, which she had seen shortly before, it did not appear to her that any great quantity had been abstracted. We were led to understand that this unfortunate woman had long complained of severe headachs, and been much troubled with uneasy feelings in the stomach and palpitation of the heart. So far as we could ascertain, however, she had not been much addicted to intemperance.

The body was inspected on the following day.

Head.—The scalp was congested with blood, particularly in the occipital region, and was easily separated from the cranium. The bone was rather thick and vascular, and more deeply furrowed than usual by vessels in the *dura mater.* The latter appeared to be thin in the greater part of its extent, but adhered firmly to the bone in several situations, and its vessels poured out a large quantity of blood upon its being separated.

There was a very large quantity of serous fluid betwixt the arachnoid and *pia mater*, the former being raised almost like a blister, while the fluid escaped freely on puncturing the membrane.

The texture of the brain presented nothing remarkable, and its vessels had a healthy appearance.

Chest.—The lungs were healthy, with a few old adhesions of the pleura. The heart was empty, and perhaps below the ordinary size, but in other respects free from disease.

Abdomen.—The mucous membrane of the stomach appeared to be rather vascular towards its cardiac extremity, and was besmeared in that situation by some whitish mucus. No smell of laudanum could be detected by any who were present, and it contained nothing but a small quantity of oatmeal in the neighbourhood of the pylorus.

When this case occurred, I was rather inclined, upon a review of the circumstances, to regard it as an instance of narcotic poisoning. I was led to this conclusion, 1. By the original suspicion entertained by the neighbours that more or less laudanum had been swallowed; 2. Because none of the appearances either before or after death were incompatible with that view of the case; and 3. From the improbability, that the morbid effusion within the head, if it had occurred idiopathically, would have increased so rapidly as to prove fatal in five or six hours.

The last of these considerations, viz. the sudden termination of the case, was evidently that which impressed me most strongly with the opinion that laudanum had been the cause of death; and

I should probably have thought so still, had not the following case, which occurred shortly afterwards, convinced me, in a very striking manner, that chronic disease may exist for months and years within the head, and at last prove very suddenly fatal, by a rapid effusion of serous fluid.

CASE III.—Mrs C., 82 years of age, had suffered a great deal from dyspeptic symptoms, but was strong, and, in many respects, healthy, till within a few months of her death. During the last four or five years, she had become extremely variable in her temper, and was generally in an unhappy discontented state of mind in the day-time,—becoming more impatient, noisy, and even violent throughout the night. On several occasions, she had been threatened with cerebral symptoms of a more serious character, which were removed by antiphlogistic remedies. On the 16th of April 1840, she appeared to be very much in her ordinary state of health, and was lifted out of bed, and placed in her chair, while the bed was being made. She was conversing with those about her, when she suddenly exclaimed, "What is this; I am very ill," and speedily became insensible, with loud stertorous respiration. I saw her in about half an hour, that is to say, at 2 P. M. She was then in bed breathing very laboriously, and with much noise, the lips being blown out at each expiration, while a quantity of phlegm rattled in the mouth and throat. When spoken to loudly, there was a slight movement of the eyelids; but this appearance of sensibility ceased in a few minutes. The pulse was rather accelerated and feeble; the skin moderately warm. A cupping-glass being applied behind the mastoid process, and a second on the temple, the blood came sluggishly at first, but gradually with more freedom, till ten ounces were abstracted. As it flowed, the pulse became slower and intermitting; the respiration at longer intervals. She died at three o'clock, just an hour and a half from the commencement of the attack.

Inspection.—The parietes of the cranium were nearly the third of an inch in thickness. The *dura mater* appeared to be thicker, and more opaque than usual, especially opposite the posterior lobe of the right hemisphere, in the neighbourhood of the cerebellum. In this situation two whitish tumours, of a fatty or suetty appearance, adhered to the inner surface of the membrane. The larger might be the size of an almond in its shell; the other somewhat smaller. They were imbedded in the brain, but not adherent to it,—two distinct depressions being observed in the latter when it was removed from the cranium. There was a copious effusion of serous fluid underneath the arachnoid membrane, where it covers the hemispheres; and the same fluid flowed in large quantity from the spinal canal, upon cutting through the *medulla oblongata.* The ventricles contained little fluid; while the *sep-*

tum lucidum, *commissura mollis*, and neighbouring parts were quite entire, and of natural consistence. The medullary substance of the brain had a yellow tint. No extravasation of blood was discovered in any part of the organ.

Case IV.—*Disease of the Cerebral Substance.*—A lady, 64 years of age, had been conscious of a gradual, though slight failure of memory, during the last three months of her life. About a month before her death, she experienced a partial numbness of her right thumb; by degrees the arm lost its power, and lay crossed upon the chest; then the corresponding leg became weakened; articulation grew indistinct; then the memory of words failed; then complete unconsciousness supervened; and lastly, coma, which existed for only a few hours previous to her death.

Inspection.—Upon separating the *dura mater*, the arachnoid seemed to be in contact with the *pia mater* throughout its entire extent, the distinction betwixt these membranes being hardly perceptible even in the situation of the sulci separating the convolutions. Both hemispheres being sliced down nearly to a level with the edge of the bone, it was remarked that the white central portion on the left side was much larger than that on the right, although its texture did not appear to have undergone any morbid change. We began to think that this might be a case of hypertrophy of the medullary substance; but farther dissection showed that the apparent enlargement was occasioned by disease. In the posterior lobe of the left hemisphere, at its lower part, was a very extensive morbid degeneration of the cerebral texture. The mass of it was soft, of a yellowish-gray colour, with small bloody extravasations scattered through it. This was bounded by a distinct margin in the greater part of its extent, being apparently walled in and separated from the healthy structure by a barrier of coagulable lymph, which felt sensibly firmer to the finger than the neighbouring parts. The left ventricle contained a tenacious substance of a yellowish-green colour, which coagulated in hot water; the right about half an ounce of clear straw-coloured serum.

The aortic and mitral valves were slightly thickened and puckered; but the heart was in other respects healthy.

The following case seems worthy of being recorded, from the apparent want of proportion betwixt the symptoms observed during life, and the morbid changes discovered after death.

Case V.—A boy in George Watson's Hospital, 10 years of age, rather delicate in his appearance, but in general healthy, became affected with pustular ophthalmia in both eyes, which proved very tedious. His general health, at the same time, became impaired; and, as the eyes got better, numerous small abscesses made their appearance in different parts of his body. He gradually improved under the use of tonic and alterative remedies, and the ophthalmia being gone, he was permitted to resume his studies. This was in the month of February.

The vision of his right eye, which squinted, had always been imperfect; but that of the left eye was good and strong previously to his late illness. He now discovered, upon attempting to read, that he could only distinguish letters of considerable size, and that even these appeared to run into each other and become confused, after a few seconds. There was no morbid appearance in either eye, except that the pupils, which were large and circular, seemed to contract and dilate more readily, and with less precision than is usual in a healthy state of the organ. In short, we had reason to fear the approach of amaurosis; and he was again brought under medical treatment. During the next month, his general health seemed gradually to improve; but his eye-sight continued much the same, although it varied considerably from day to day, as he could sometimes read several lines of moderately small print with little difficulty; while, on other occasions, not a single letter could be distinguished. He complained of occasional pains in his head, and sometimes, I believe, of giddiness; but was remarkably shrewd and intelligent for his years.

On the 21st of April he went home, and was placed under the care of my friend, Dr James Combe. His symptoms underwent little or no alteration; but he became gradually weaker, and died very suddenly on the 11th of June, while preparing for a walk in the open air.

Through the kindness of Dr Combe, I was present at the *post mortem* examination. There were seven or eight scrofulous tubercles, of various sizes, in different parts of the cerebrum and cerebellum. These were firm in their external surface, and could be easily dislocated from the substance of the brain. Though imbedded in the medullary portion, they seemed to be all connected more or less with the *pia mater.* On being divided, they presented a greenish-yellow section. The ventricles contained a considerable quantity of limpid serum. The optic nerves had a healthy appearance.

I shall venture to relate the following case, because it affords a satisfactory illustration of sanguineous apoplexy, both in its causes and effects.

CASE VI.—Miss R. was about 40 years of age when she died, and had been an invalid for more than six years, with symptoms referable to the heart. From her childhood she was easily put out of breath, and was liable to palpitations. These symptoms seemed to become gradually more decided; she occasionally awoke in the night-time breathless, and was obliged to sit up in bed from a sense of suffocation. Still, however, she appeared to be in robust health, was remarkably energetic, and took a great deal of exercise in all weathers, eating and drinking as usual, in spite of the remonstrances of her friends, who observed that she was becoming stout and more plethoric.

While returning from one of her long walks in the spring of 1833, she was suddenly attacked with a severe pain and oppression in the chest, and became totally incapable of proceeding another step. She was bled very freely, and, after a good deal of treatment, the heart's action, which had been very violent, became quiescent; that is to say, the more alarming symptoms subsided; but the least excitement of the circulation threatened to revive them.

From this period she was put upon an antiphlogistic regimen, and enjoyed a considerable share of good health for several years. By degrees, however, she began to take liberties—went out in unfavourable states of the atmosphere, and in this way caught a succession of colds, each of which seemed to give fresh energy to the latent disease in the heart. Towards the end of summer 1838, her temper, which was at all times imperious and irritable, was a good deal tried by domestic annoyances. These excited her circulation; and one morning early in August, while in the act of dressing, she had an apoplectic attack, with paralysis of the right side. The coma and insensibility soon went off, and she then distinctly referred all her uneasiness to a point in the head, corresponding with the left side of the frontal bone, near its connection with the parietal. Her mind was a good deal weakened by this attack, and her articulation rendered very indistinct. The paralytic symptoms diminished by degrees; but never entirely disappeared.

The disease in the heart seemed to advance steadily from this period. Its action was at once impetuous, extensive, and frequent, while the pulse at the wrist became gradually smaller and less frequent, till at length it could hardly be felt, and did not beat oftener than thirty or forty times in the minute. It seemed as if four or five efforts of the heart were necessary to induce a single systole of the arterial trunks. Her countenance became more and more pinched, with a peculiar yellowish and cadaverous tinge of the skin. The appetite entirely failed; and she sunk so gradually at last, that life had probably been extinct for some time before the suspicions of those about her were awakened. She died in August 1839, twelve months after the apoplectic seizure.

Inspection—Head.—The scalp was remarkably bloodless; the bone very dry, dense in its texture, and thicker than usual. The *dura mater* separated very readily from the cranium. Several ounces of a clear serum escaped from the cavity of the arachnoid. It had collected chiefly at the base of the brain; and there was also a small quantity of fluid betwixt that membrane and the *pia mater*. The ventricles did not contain much fluid, although the foramen of Monro was twice its ordinary size. The *septum lucidum* and neighbouring textures were entire. The cerebral substance appeared healthy, save in the immediate vicinity of the left lateral ventricle, at its outer margin, where there was a small cavity or cyst about three-quarters of an inch long, and half an inch broad, separated from the ventricle by a very thin, almost transparent, partition of medullary matter. This cavity contained a small quantity of whey-like fluid, was intersected by several delicate bands, apparently of lymph, and presented a yellowish-gray appearance upon its inner surface. This lining substance, however, could hardly be described as an organized membrane.

Chest.—The pericardium adhered almost universally to the surface of the heart, but admitted of being detached, as if connected to it merely by cellular tissue. The heart itself was greatly enlarged, flabby, and easily torn; the right side dilated, with very thin parietes; and the auriculo-ventricular orifice considerably enlarged. The left ventricle was also enlarged, and its parietes at least half an inch in thickness. The mitral valve thickened, fleshy, and defective.

The lungs were loaded with frothy serum, the left adhering extensively to the *pleura costalis*. The venous trunks in the neighbourhood of the heart were loaded with black fluid blood, which escaped in large quantity from the sinuses within the cranium, during our subsequent examination of the chest.

I shall finish this communication with three cases of cerebral congestion, which occurred about the same time, and terminated favourably.

Case VII.—A hale-looking man of 60, tall, and by no means inclined to corpulency, went to his work, as a bookbinder, at five o'clock in the morning, in perfect health. Shortly afterwards he was seized suddenly with a swimming in his head, and dimness of vision, succeeded by shooting pains in the head. These symptoms, which were not accompanied by the least confusion of thought or paralysis, continued with little alteration till I saw him in the afternoon of the following day. He then complained of headach, impaired vision, being quite unable to read, and a tendency to sickness upon sitting up in bed. His wife remarked a peculiar expression of his eyes. The pulse was strong, full, and incompressible. The tongue clean and moist.

Two pounds of blood were taken from his arm in the sitting posture, without the supervention of syncope; and as it flowed the head became easier, and vision improved.

Several active doses of purgative medicine completed the cure,—so that two days after the bleeding he complained only of weakness.

Case VIII.—A man, 36 years of age, a cook and confectioner by trade, was seized suddenly with a pain in the forehead, and a rushing sensation through the head. I did not see him till three days had elapsed. The sensations in his head continued much the same; he had some unsteadiness in walking, and complained of seeing animals of all shapes and sizes before his eyes. He was quite free from delirium, and gave a distinct and connected account of his feelings. The pulse was full and firm, but not accelerated; tongue slightly furred; the bowels had been freely moved by medicine; the evacuations offensive, pale, and lumpy.

A full blood-letting from the arm, which caused faintness, relieved, without removing entirely the unpleasant sensations in his head. The blood was sizy and cupped. He was then freely purged, and had leeches to the temple; but did not experience much benefit till the head was shaved, when all the symptoms disappeared almost immediately. He felt himself quite well, on the second day after the commencement of the attack.

Case IX.—A gentleman, 36 years of age, in the enjoyment of robust health, and extremely active in his habits both of mind and body, but inclined to corpulency, was engaged in reading aloud to his family, when his eye-sight became suddenly so imperfect that he was unable to proceed. He at the same time exhibited slight incoherence for a few minutes, forgetting where he was, and employing one word instead of another.

I saw him in about half an hour. He was then reclining upon a sofa, and gave a clear and connected account of what had happened; but, in doing so, made several verbal mistakes, of which he was perfectly conscious. His countenance was flushed, with a dull expression of the eyes; the pulse 75, full, and soft. He complained of an uneasy sensation, hardly amounting to pain, in the top of the head; of pain on coughing in the posterior part of the left orbit, and of a slight feeling of numbness in the tongue and right arm,—but there was no want of muscular power in either of these parts. He tried again if he could see to read, but was unable to distinguish a letter.

Thirty ounces of blood were taken from his arm, upon which the uneasiness in the head disappeared, and vision became as distinct as usual. The bleeding did not produce syncope; but the flushing subsided, and his natural expression returned.

The numbness of the tongue and right arm, and the pain behind the left eye on coughing continued for some hours, and then took their departure, after the bowels had been powerfully acted upon by purgative medicine. Four or five days afterwards, having imprudently taken up a book, and allowed his feet to get very cold, he again experienced a slight numbness of the tongue, with a tendency to use one word for another. These symptoms went off in about half an hour, and he has had no return, although nearly eighteen months have elapsed since the attack.

I ascertained that he had been affected in the same way, although less alarmingly, upon four previous occasions. One of these was at a party before and during dinner, at a time when he had been much engaged in public business of an exciting nature. The illness was very transient, and consisted in mere forgetfulness of words for a few minutes. Another attack came on while he was engaged in copying a paper, so that he wrote ten or twelve lines of absolute nonsense, which he still keeps in his possession as a record of the temporary indisposition. It lasted, in fact, no, longer than the time occupied in the mere manual process of writing the lines in question, which were both preceded and followed by coherent and well-written sentences. He is a man of regular and temperate habits, but had a custom of eating very fast, and drinking large quantities of water, while he entered with great zeal and enthusiasm into all his undertakings. Since the last attack, he has endeavoured to take every thing more coolly, being conscious, from his own feelings, that he can no longer undergo the same excitement as formerly with impunity.

The three foregoing cases have been related as instances of cerebral congestion; understanding by that term, a disturbance of the relative proportion which obtains during health betwixt the arterial and venous systems of the brain; because it seems very evident, from the experiments and reasonings of Drs Kellie and Abercrombie, that the peculiar structure of the cranium prevents the entire quantity of blood within it from being materially increased or diminished under any circumstances. The quantity of arterial blood may be casually increased; but then there will be a corresponding diminution of the venous, and *vice versa*, an increased proportion of venous blood will necessarily be accompanied by a deficiency of arterial. Either of these deviations from the healthy standard may be regarded as cerebral congestion,—but they proceed from different causes, and, within certain limits, may demand different remedies. For example, when the symptoms of cerebral congestion are attended by a strong and full pulse in the arteries of the neck, we may confidently infer that the balance betwixt the two systems within the head has been disturbed by an excessive flow of arterial blood, and is to be mainly restored by reducing the impulse of the general circulation. It appears to be in this way that blood-letting and other antiphlogistic remedies effect a cure in cases such as those which I have related. But, again, the symptoms of congestion may be accompanied by a feeble pulse, while the great veins in the neck are evidently distended and oppressed; in this case, we may infer that the contents of the cranium have more than their healthy allowance of venous blood. Under these circumstances, also, blood-letting may be useful to a certain extent; but we cannot have recourse to it with the same confidence, and must beware of increasing the evil, by depressing still further the already enfeebled arterial circulation.

ART. XIII.—*Report of the Labour of Mrs C., in whom the neck of the Uterus was amputated by Professor Simpson*, By ROBERT LEWINS, Junior, M. D.

A history of the progress of this case since the date of Dr Simpson's last report, recorded in the number of the Edinburgh Medical and Surgical Journal for January of the present year, will, I have no doubt, prove interesting to the profession.

From the 14th November until the 19th December, Mrs C. remained in good health; but on that day she was attacked by violent pain in the abdomen, which occasioned alarm, and rendered it expedient to have recourse to blood-letting, and other active measures, by which the urgent symptoms were speedily removed. From this period, until February 15th, she remained quite well. On the evening of that day, my father was requested to visit her. He found the membranes ruptured; and that there had been a considerable discharge of *liquor amnii*, tinged with blood. On making an examination, the uterine aperture was found to be as large as a sixpence; the parietes of the organ, as far as the finger could reach, being very thin, tense, and rigid. The presentation of the child was ascertained to be natural. No labour pains had as yet come on; nor did they commence during the subsequent night. About half-past ten on Tuesday evening, however, (16th February), they became pretty smart and regular, and from this period she may be considered to have been fairly in labour. On being summoned about two o'clock on Wednesday morning, I found the opening into the uterus about the size of a shilling, of an oval shape, the long diameter of which was in a line leading from the right iliac synchondrosis to the left acetabulum; slight oozing of the *liquor amnii* was still taking place, notwithstanding which, the bag of membranes distended with fluid, was protruded in front of the child's head during every pain. This I could only account for, by supposing that the place at which it had given way had been displaced from the situation it had formerly occupied opposite the aperture in the uterus, before all the fluid had been evacuated. The edges of the *os uteri* were very rigid, sharp, and tense, conveying to the finger the sensation of a tendinous ring, upon which the labour pains, although very regular, produced little or no effect in the way of dilatation. Matters remaining in precisely the same condition at five A. M., I had recourse to artificial dilatation of the *os uteri*. which was persevered in cautiously, and at intervals during the pains, with the very best effect. Considerable progress had been made at eight o'clock, when the uterine orifice was as large as a half-crown piece ; the edges, however, still continued so rigid and tense, that during every pain I feared laceration would occur.

Since manual interference had been resorted to, the pains had been violent; and about nine o'clock A. M. a feeling of cramp being complained of in the abdomen, during the absence of the ordinary uterine contractions, I took away some blood from the arm, which was followed by immediate and complete relief. Dilatation was continued at intervals throughout the forenoon, and very slow and gradual progress was made with the labour, the patient complaining greatly of pains in the back and thighs. On several occasions during, and immediately after attempts at dilatation had been made, she had a fit of vomiting, which was also excited on her assuming the sitting posture. During all this time she preferred remaining upon her back,—her sufferings appearing to be more supportable in that position than in any other. The vagina and *os uteri* were well lubricated with axunge. Early in the afternoon the pains assumed more of an expulsive character, the anterior edge of the uterine aperture being protruded, rigid, and unyielding in front of the child's head into the vagina. About half-past three, the pains increased in severity and duration, but no further dilatation of this portion of the uterus took place. A few minutes before four, however, I succeeded in slipping this impediment over the child's head, which immediately afterwards, during a prolonged pain, was expelled from the *os externum*. The remainder of the process went on without difficulty. The infant seemed to have suffered somewhat from pressure, but, upon being sprinkled with cold water, it completely revived. Soon after the birth of the child, a pain expelled the placenta into the vagina, from whence it was easily removed ; the whole process being thus completed in seventeen and a-half hours from the first accession of regular labour pains.

The child was healthy and vigorous, and although unquestionably before the full time, the operation of excision of the *os* and *cervix uteri* having been performed on the 25th of May last, presents no marked deviations from a nine months' child, its weight being seven pounds, and its length being 19½ inches. Mrs C. was troubled with severe after-pains, for the relief of which an opiate draught was prescribed, in consequence of which she enjoyed a good night's rest, and on the following morning was fully as well as she usually is on such occasions.

Her progress since has been uninterruptedly favourable; her convalescence was rapid; she is at present in the enjoyment of perfect health, and quite fit for her duty as a nurse.

In the annals of medicine there are few cases recorded which afford more striking and gratifying proof of the triumph of our art over a disease, which, from its nature and seat, had by British surgeons been considered desperate and beyond the reach of human aid.

There is, however, reason to suppose, that the illustrious Ambrose Paré contemplated the possibility of performing this bold operation; and Lauvariol, a French surgeon, actually proposed it in 1780. We find Wrisberg, Schlessing, and Krevel in Germany, and Monteggia in Italy, soon afterwards advocating the propriety of excising the *cervix uteri;* but there is no evidence of its having been performed on the continent until 1801, when it appears to have been successfully practised by Osiander, Professor of Midwifery in the University of Gottingen; and in a paper communicated by Dr Thomson, our present distinguished Professor of Pathology, and published in the number of this Journal for July 1816, there is a minute and interesting statement of Osiander's mode of operating in nine successful cases.

Soon after the publication of Osiander's paper, my father, then a young practitioner, suggested to the late Mr Welsh of Haddington, the expediency of an attempt to excise the *cervix uteri* in the case of a lady of high rank, who laboured under malignant disease of that organ,—a proposal which at that time was considered in Scotland preposterous or something worse. Osiander informs us, that, according to the testimony of all who have submitted to this operation, it is not nearly so painful as one might imagine. Other continental operators have expressed themselves to the same effect; and this fact was also verified in the case of Mrs C.

Dupuytren, Recamier, Colombat de l'Isère, and Lisfranc, are the principal French surgeons who have distinguished themselves as operators in this department. The latter asserts that he has performed the operation of section of the womb ninety-nine times; the accuracy of that statement, however, has been called in question by his countryman and ex-premier prosecteur, M. H. Pauly, who nevertheless, admits that Lisfranc was eminently successful in one case, that of Madame Carpentier, which forms, according to him, "la gloire vivante de l'amputation du col de l'uterus, c'est elle que depuis son operation a eu quatres enfans dont deux jumeaux"—a case which, to the honour of Scottish surgery, now no longer stands unrivalled.

With Drs Churchill and Simpson, I am ignorant of any attempt which has been made in Great Britain before the present case to excise the *cervix uteri.* Dr Davis, it is true, in his learned work on the Principles and Practice of Obstetric Medicine, refers somewhat vaguely to its having been performed with at least temporary success in England. In 1741, (just one hundred years ago), Mr Pugh of Chelmsford removed with perfect success during parturition, a large "fleshy excrescence that grew on the edge of the *os tincae*." (See his Treatise of Midwifery, p. 122.) Mr Bell extirpated a polypus from the same locality during labour, (Edin. Med. and Surg. Journ. Vol. xvi.); and Levret, Gooch, &c., have given drawings and cases illustrative of such pediculated polypi and excrescences of the cervix. Professor Syme and others have removed tumours of the same nature from the unimpregnated *os uteri*; but these I conceive to have been very different operations from excision of the neck and part of the body of the womb.

The successful result of the cases referred to have demonstrated an important physiological fact, that the presence of the cervix and *os uteri* are not indispensable to the existence and due consummation of pregnancy. Lisfranc, indeed, remarks that labour is more easy after the removal of these parts; but in this he is assuredly mistaken. The cicatrized condition of the uterine aperture is calculated to render its dilatation exceedingly tardy and painful; and in our case continued artificial assistance was required to accomplish the object, generally easily effected by the unaided efforts of nature; and, notwithstanding, this artificial aid, the first stage of labour was not completed until after a lapse of upwards of seventeen hours, although the patient had previously born five children, her last labour having only lasted two hours.

In another point our experience does not coincide with the representations of Lisfranc, who informs us that the wound of the uterus is not favourably circumstanced for quick cicatrization, to promote which, he recommends first, injections of emollient, afterwards of stimulating fluids, and subsequently cauterization with the liquid protonitrate of mercury. The incised surface in Mrs C.'s case, when examined through the speculum a few days after the operation, presented a healthy granulating appearance; and it is certain that conception took place within ten days from the date of the operation.

In conclusion, I would earnestly insist on the importance of early and accurate examination of the uterus by the speculum in all cases of suspicious vaginal discharges. The result in Mrs C.'s case is to be attributed to the precise nature of the disease being promptly ascertained. Cases of a similar nature are, I fear, frequently treated by the routine practitioner as simple menorrhagia; whilst the malady is insidiously gaining ground until the performance of an operation no longer affords a rational prospect of relief. The ill-judged opposition to the employment of the speculum has contributed to create, and confirm, a prejudice against the use of that instrument, which may probably, in some measure, explain why we are deficient, when compared with our continental brethren, in the knowledge and treatment of uterine diseases; a reproach which the precepts and example of Professor Simpson, as triumphantly manifested in the result of Mrs C.'s case, will, I trust, tend materially to remove.

P. S.—Since writing the above remarks, I have been favoured by Dr Simpson with the perusal of a communication to him from Dr Ingleby of Birmingham, in which that distinguished accoucheur relates two interesting cases in which he performed the operation under consideration. "I once amputated," says Dr Ingleby, "the *cervix uteri* for cauliflower growth. It was unaccompanied by pain, as these cases always are. Hemorrhage, serous discharge, dropsy of the extremities and face, with general anæmia, were the prominent signs. All the disease was removed which was connected with the uterus. Small bits, however, grew from the mucous membrane of the vagina. Whether caustic would have succeeded in eradicating these, I am unable to determine, as thoracic inflammation came on a few days subsequent to the operation, and the patient died from it and the effects of a very large vomica in one lung. Every part of the body was sound except the lungs and the mucous membrane of the vagina, just below the cut surface of the *cervix uteri*, and opposite the *os* and vaginal portion of the organ. I once also excised the *cervix uteri* for a bleeding cancerous fungus, which did not extend above the *os uteri* more than a quarter of an inch. I removed all the disease, and likewise some of the sound parts. The patient, who was almost moribund prior to the operation, became apparently quite well, got actually fat, and remained in good health for a year. The disease then returned, chiefly in the vagina and bladder, in consequence of which she died."

Dr Ingleby also relates, cursorily, a case in which he witnessed this operation performed, without, however, arresting the progress of this dreadful disease.

4, *Quality Street, Leith,*
8th March 1841.

PART II.

CRITICAL ANALYSIS.

Art. I.—1. *Researches in Embryology.* First Series. By Martin Barry, M. D. F. R. S. E., Fellow of the Royal College of Physicians of Edinburgh. (Transactions of Royal Society for 1838, Vol. cxxviii. Part I. p. 301.)

2. *Researches in Embryology.* Second Series. By Martin Barry, M. D. &c. (Transactions of Royal Society for 1839, Part I. p. 307.)

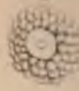
3. *Researches in Embryology.* Third Series. *A Contribution to the Physiology of Cells.* By Martin Barry, M. D. F. R. S., F. R. S. E., Fellow of the Royal College of Physicians of Edinburgh. (From the Philosophical Transactions for 1839–40.)

We still entertain the opinion, in spite of what has been said by Mr Wharton Jones, that the subject of these memoirs has been little attended to by the physiologists of this country, in comparison to what it has been in some parts of the continent; and, with every wish to give due credit to Mr Jones for his writings on the subject of embryology, we cannot but look upon Dr Barry as the first original observer in this country who has trodden the obscure pathway to the history of Organized Beings.

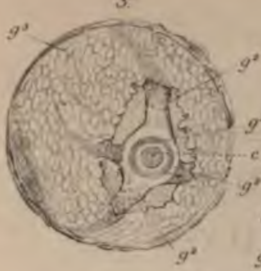
It is rather a remarkable circumstance, and one which clearly demonstrates the general attention which this interesting subject is attracting, that the Institute of France allotted their prize for physiology, and chiefly for this department of it, to Dr Rudolph Wagner, in the same year, and nearly about the same time that the Royal Society of London awarded to Dr Barry "The Royal Medal in the department of Physiology, including the natural history of Organized Beings," for his papers entitled "Researches in Embryology," published in the Philosophical Transactions for 1838 and 1839.

This circumstance of itself sufficiently shows the very general attention which physiologists are devoting to this subject; and that, while we were once behind our continental brethren in this department, we now stand upon an equal footing, if we have not even taken a step in advance. From these circumstances, and from

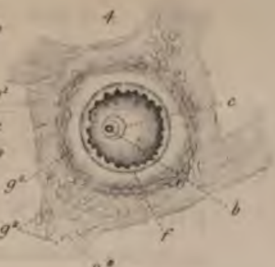
Fig. 1.



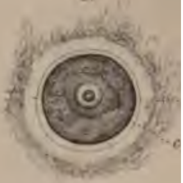
3.



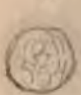
4.



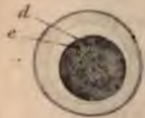
5.



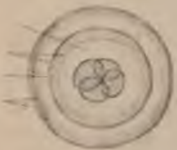
2.



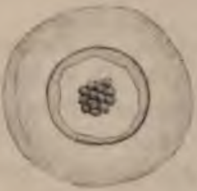
6.



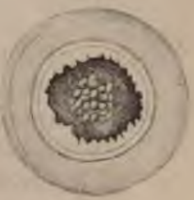
7.



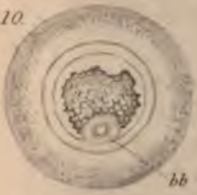
8.*



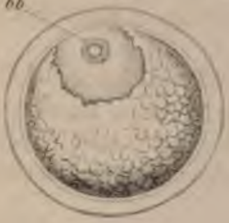
9.



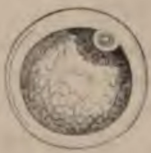
10.



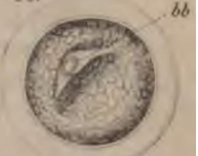
11.



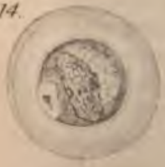
12.



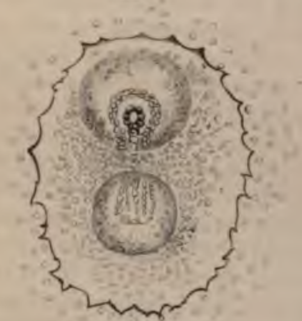
13.



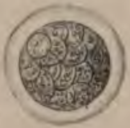
14.



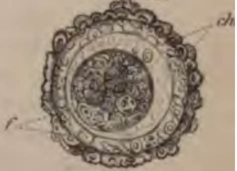
15.



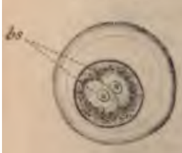
16 or 156.



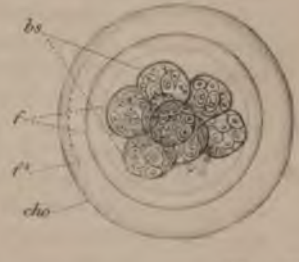
17 or 185.



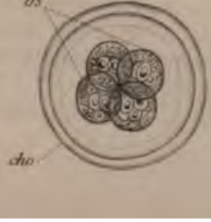
18 or 191.



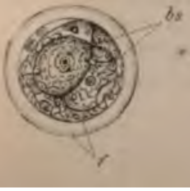
22 or 212.



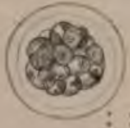
20 or 210.



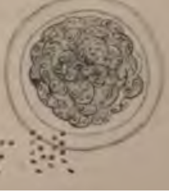
19 or 200.



21 or 229.



23 or 231.



the very original nature of Dr Barry's memoirs, we think it indispensable to place before our readers an account of the researches made, as fully as is consistent with the pages of this Journal.

We owe to the celebrated Regnier de Graaf the first notion, that the ovum of the mammalia exists already formed in the ovary. Although it may be considered doubtful whether Steno was the originator of the doctrine, De Graaf at least, by a series of well-conducted experiments and sound reasoning, came to the conclusion, that the ovum of the mammalia must exist already formed in the ovary. From De Graaf's experiments and reasoning, the following results were obtained. That the ovaries are the seat of conception; that the number of vesicles which enlarge correspond to the number of foetuses formed;[*] and that these vesicles discharge, at a certain period after conception, a substance, which soon appears under a vesicular form in the Fallopian tube, (he having found two distinct ova there.) De Graaf, however, had the misfortune to have his testimony on this point doubted, as various other authors, and amongst them the celebrated Baron Haller, searched many times, and in vain, for similar objects; and further, as when puncturing an enlarged ovarian vesicle, nothing was observed to escape, but a drop of albuminous fluid, De Graaf's opinion seems to have been nearly abandoned, and the one which Haller countenanced generally adopted; viz. that the ovum was formed from a drop of the fluid of the ovarian vesicle, while passing along the Fallopian tube to the uterus. Mr Cruikshank, from his experiments, came to similar conclusions with De Graaf more than a century afterwards; and he has delineated very accurately the appearance of the ova he met with in the Fallopian tubes. Prevost and Dumas, in 1824, obtained a glimpse of something that must have been the ovum while still in the ovary. Von Baer, however, in 1827, found and recognized it there; a circumstance which justly forms an epoch in the history of the developement of the ovum.

Many of Professor Von Baer's countrymen have followed close upon him in this interesting branch of scientific inquiry, among whose names may be mentioned those of Professors Purkinje, Krause, Valentin, R. Wagner, &c. and M. Coste in France. We know of no author having devoted himself to the subject of these memoirs in this country previous to the time of Dr Barry. We may mention, however, as contemporaries in the field, the names of Mr T. W. Jones and Dr Allen Thomson. The author of the present series of observations, however, has very far outstripped his contemporaries, and has already made for himself a path through this intricate ground, but which he has left clear and obvious; and in which it is now our intention, as closely as possible, to follow him.

---

[*] We shall afterwards have occasion to mention, that Dr Barry has completely disproved this assertion of De Graaf's; and has shown, that a great many more vesicles enlarge than those which burst and allow ova to escape.

Dr Barry has divided the consideration of the history of the mammiferous ovum into three heads, each of which occupies one of the series. In the *first* he considers its several periods of formation; in the *second* he traces it through the early stages of developement; and in the *third* he discovers to us a remarkable process of developement by cells. We shall follow, then, our author in this order, and consider the mammiferous ovum in its several periods of formation. But, first, it is requisite to make a few observations regarding the word ovum.

It is well known that the word ovum was first used by De Graaf to designate the vesicles of the ovary. When Von Baer discovered the more minute and proper body within the ovum, he called it ovulum, and for a long time, and even yet, this object is generally known under the term ovule of Von Baer. Dr Barry has again changed the name of this body, and gives to it the term ovum, in consequence of his having demonstrated "that the chorion or external membrane of the ovum of the uterus is a primitive part of the ovarian vesicle of Baer. It is perhaps better to call the latter an ovum, as will be done in this memoir."

We entirely coincide with our author in this change of name, equally for the reasons he has assigned, and from the fact, that the word ovulum is a diminutive, and consequently leads to the idea that there must be a larger body, an ovum, while this minute body is called ovulum. The ancient ovum now goes by the name of the Graafian or ovarian vesicle, the ovulum of Von Baer, the ovum.

Professor Von Baer discovered the ovum of the mammalia, as has already been said, in 1827, and conceived that it corresponded to the germinal vesicle of other animals. Purkinje questioned this analogy; and it was soon shown how far this last named author was right, by the discovery in 1834, by Valentin and Bernhardt, of the germinal vesicle in the ovum of the mammalia. In the same year another addition was made to our knowledge of this body, by Professor Rudolph Wagner having discovered the *macula germinativa*, or germinal spot, on the internal surface of the germinal vesicle.

Ovarian vesicles are generally described by authors to possess two membranes, an internal and external. Dr Barry, however, dissents from this doctrine, and considers the internal membrane as the ovisac, which, as it enlarges in its primitive vesicular form, attaches to its outer part a quantity of the cellular tissue of the ovary, and thus constitutes the external membrane of the Graafian vesicle in its mature state.

The early structure of the ovisac in the mammalia may be seen either in quite young animals, or in those that have lately reached the state of puberty, and originates as a vesicle in the proper substance (stroma) of the ovary. "Its general appearance when first formed is as a minute, pellucid, often yellowish sac, having an elliptic form, and plaits or folds in its contour." The ovisacs are generally found in groups, and their minuteness when first formed is almost incredible. Dr Barry has found them in the ox, measuring not more than the fiftieth, and in the dog only the hundredth of a Paris line.

" The only objects which in general are visible in the ovisac when the latter is first formed are a few granules. These granules deserve a particular description from their peculiar appearance—from their entering into the formation of several important structures to be afterwards described,—and from their being very intimately connected with the evolution of the ovum, from its early appearance as the germinal vesicle, until, as a mature ovum, it has passed into the Fallopian tube."

These granules are elliptical, sometimes nearly round, and generally flattened, exceedingly transparent, often punctate, and presenting with more or less distinctness a nucleus. " In the substance of this nucleus, when highly magnified, there is observable a point still darker." These granules vary in size from the four hundredth to the one hundredth of a Paris line.

The fluid which is contained in the ovisac is generally considered to be of an albuminous nature ; it is partially coagulable by heat and other agents, and contains a large number of the peculiar granules just described. It is a curious fact regarding these granules, that water dissolves them while serum does not.

Amongst the peculiar granules of the ovisac which we have just been examining lies a body which, supposed to be the most important, also appears to be the most primitive element of the ovum, viz. the germinal vesicle. Purkinje, the discoverer of the germinal vesicle, has expressed his opinion, that it is the first part of the ovum formed. Baer observed its formation preceding the evolution of the ovum in vertebrata and mollusca. R. Wagner has found them in insects, and Dr Barry has considerably extended the observations of Baer and Wagner, more especially to two classes of the vertebrata, the rabbit and pigeon. " From the observations of Professors Baer and R. Wagner in invertebrated animals, and those now recorded from researches in two classes of the vertebrata, I think I am warranted in concluding, that the germinal vesicle and its contents constitute throughout the animal kingdom the most primitive portion of the ovum." The next process in the order of formation is the accumulation around the germinal vesicle of the peculiar granules of the ovisac and oil-like globules, then the ovisac is formed. Fig. 1st and 2d of Plate XI. represent the appearances of the granules of the ovisac and the germinal vesicle, in the rabbit, and the human being. Liquefaction of some of the granules now appears to take place, (or a fluid from some other source is added,) and the germinal vesicle is then seen in or near the centre of the ovisac. " Subsequently a covering or tunic, consisting of a kind of dense cellular tissue, susceptible of becoming highly vascular, and closely connected with the surrounding stroma, is gradually formed upon the outer surface of the ovisac, which, previously in a high degree transparent, now becomes translucent only." Thus is constituted a mature ovum.

Dr Barry then occupies some space in details regarding the similarity which he attempts to show between the ovisac of birds, amphibia, and fishes, and the ovisac of mammals. This is a subject which seems to have been much misunderstood even by very recent writers. The ovisac of birds, amphibia, and fishes, was generally looked upon by authors as corresponding to the chorion of the mammalia. "The ovarian calyx of the bird," says Dr Barry, "if deprived of its peritoneal investment, and what there is of the parenchyma of the ovary, would, I apprehend, present a structure analogous to the Graafian vesicle of mammals, that is, an ovisac which has acquired a proper (and in the latter stages a highly vascular) covering." "The ovisac of amphibia and that of fishes, improperly called the "chorion," first presents itself in a very similar condition to the ovisacs of mammalia and birds." "The chief difference consists in the fluid of the ovisac not generally (though in some instances,) presenting the peculiar granules described as occurring in mammalia and birds." We wholly agree with Dr Barry in the analogy he has made out, and conceive that the beautiful and minute drawings with which he has illustrated the subject, proves it to demonstration.

The proper membrane of the yelk in mammals next occupies attention. It is so delicate as not to be easily distinguished. It has been figured by Krause in the goat. Professor R. Wagner seems convinced of its existence throughout the mammalia, and Dr Barry thinks that the distinct line, which is generally regarded as representing the internal surface of the thick chorion, may be partly due to this membrane. Dr Barry has noticed that in the rabbit, *post coitum*, and just before the ovum leaves the ovary, the membrane of the yelk, previously so thin, becomes perfectly distinct, and very thick.

We have thus seen that, in the four classes of animals we have been considering, a perfect analogy exists between the rudiments of the ovum; that in all we have seen the germinal vesicle with its contents, the yelk, and proper membrane of the yelk. Here, however, the analogy is terminated by the formation within the ovary in mammalia, of a membrane which appears to be the true chorion. This is generally allowed to be wanting in all other classes of animals.

"The chorion then, as it exists in the ovary, is soft, very thick, and remarkably transparent. It closely invests the *membrana vitelli*, so as in nearly all instances to conceal the separate existence of this very delicate membrane." "I find, that, just before the ovum leaves the ovary, this membrane begins to be attenuated by imbibition of fluid into its interior, and consequent distension." "The great thickness and transparency of the chorion, as it exists in the ovary, long prevented its real nature from being ascertained, and appears to have been the means of misleading several excellent observers.

The Tunica Granulosa.—When a Graafian vesicle is ruptured under the compressor, the ovum escapes, and with it a mass of granules (the proligerous disc of Baer). This structure, from the rugged way in which it escaped, was generally looked upon as lacerated. "I find, however," says Dr Barry, "that, if the mass thus escaping be attentively examined, those granules that immediately surround the ovum appear to be in a state of denser aggregation than the rest, from which they are to be distinguished, therefore, by a circumscribing line." "If the granules thus circumscribed be received at different distances, they are seen to invest the whole surface of the ovum, forming a tunic, which is perfectly spherical in form." "I have frequently obtained the ovum invested by no other granules than those of this tunic; that is, this tunic has been freed from the other granules, which, on the bursting of a Graafian vesicle, generally escapes adherent to it; the fact being, that all that properly belongs to the ovum of the mass of granules, in which it is found imbedded in the Graafian vesicle, is this tunic, between which and the surrounding mass there is much less adhesion than there is between the granules of either among themselves." "Having never failed to find this tunic enveloping mature ova in the mammalia, I believe its presence to be constant and essential." Dr Barry calls it the *tunica granulosa*, or granulous tunic of the ovum. The formation of this granulous tunic appears to take place just after the formation of the chorion.

The peculiar granules of the ovisac or Graafian vesicle, form the *tunica granulosa*, which has just been described; but, besides this, they form the *membrana granulosa* of Baer, a structure which invests the internal surface of the Graafian vesicle, and, besides these two, they also form prolongations or chords, which stretch from the *tunica* to the *membrana granulosa*. These our author has the merit also of first describing. He calls them Retinacula, and thus describes their use. "They obviously suspend and retain the ovum in its situation in the fluid of the Graafian vesicle," and when the ovum passes from the centre of the Graafian vesicle to its periphery, the change of position is accomplished by means of this structure. "What is very remarkable, and an interesting instance of design," is "the particular part of the periphery of the Graafian vesicle to which the ovum is thus conveyed is always that directed towards the surface of the ovary." "Nor does the office of the retinacula end here." Having suspended the ovum in the centre, and conveyed it to the periphery of the Graafian vesicle, they now retain it in the latter situation.*

Such, then, are the principal facts which our author has made out regarding the appearances presented by the ovum of vertebrata in its several periods of formation. We have now brought it the length of maturity, ready to receive the influence of impregnation. The object, then, of the next head, the second series of Dr Barry, is to trace the changes which occur in the mammiferous ovum, from the time that the coitus takes place to the incipient appearance of the vertebrae. Little or nothing was previously known of the changes which take place during this interval. Dr Barry found the field blank, and it has been his object to fill it up. This, we are convinced, he has done with great ability, and to the satisfaction of every scientific inquirer. His opportunities of doing so have been extremely numerous, and each of his experiments have been repeated so often, that the utmost confidence may be placed in the results. "The number of rabbits examined," says he, "considerably exceeds one hundred, and I have recorded in my notes the particular results regarding eighty-nine." "Besides ova that were still present in the ovary, and apparently destined to escape, 93 have been found in the Fallopian tube, and 236 in the uterus." But the difficulties which on all sides surrounded this subject, when Dr Barry took it up, will be best drawn from his own words.

We are well aware, that in the *rut*, or heat, as it is frequently called, the parts of generation become extremely vascular; Graafian vesicles become enlarged; and Dr Barry has found, contrary to the observation of De Graaf, that more of them become enlarged than are really impregnated.

"I have found it extremely difficult," says he, "to distinguish with precision between the changes referable to the rut, and the further changes resulting from the coitus. For reasons given in the introduction to this memoir, it did not seem sufficient to see the state of ova first in the Fallopian tube.† But then it was equally unsatisfactory to examine ova still in the ovary, for at first I was not in possession of any means of distinguishing those that were really destined to be discharged, as, in addition to the Graafian vesicles, from which ova would have been expelled, I have, as already stated, generally found several others that had become enlarged and vascular. It, therefore, was a great object to meet with ova that were on the point of entering the tube.

"The periods of four, six, eight, and eight hours and a half *post coitum*, were found too short, the ova being still within the ovary, and apparently not very near the time of their expulsion. A rabbit was examined at eleven hours, when the ova were found to have made their exit from that organ; and one at ten hours, with the same result. Another was killed at nine hours. The ova were still within the ovary, and their Graafian vesicles presented no decided indication of an approaching rupture. I tried nine hours and a quarter, when the ova were again found within the ovary, while in another instance, at nine hours, they had escaped. This was discouraging, but it seemed worth while to persevere. At length, after nearly a score of rabbits had been devoted to anatomical inspection, for the single object of determining the condition in which the ovum leaves the ovary, the parts were found in a state precisely what I had desired so much to meet with."

---

* Figure 3d represents the ovum of the rabbit with the *tunica granulosa*, (g^1); Retinacula, (g^2); and *membrana granulosa*, (g^3), part of which is removed; (germinal vesicle, (c).

† The changes which they undergo are so rapid and numerous after their escape from the ovary.

According to Dr Barry's observation, the ova of rabbits are generally discharged from the ovary in the course of nine or ten hours *post coitum.*

Changes which take place on the ovum, the result of impregnation, while still in the ovary.—" After some hours, the germinal vesicle is found to have left its situation at the surface of the yelk, and to be returning to its centre, from whence it came." About the same time, a vesicle arises from the surface of the germinal spot, and, speedily enlarging, applies itself to the inner surface of the germinal vesicle. This vesicle, of course, as it enlarges, imbibes the fluid of the germinal vesicle. The germinal vesicle, therefore, has now two membranes.

" The germinal spot, previously on the internal surface of the germinal vesicle, is soon observed to occupy its centre, presenting thus the same changes in relation to the germinal vesicle as the latter undergoes in reference to the yelk-ball." " The spot becomes very much enlarged, and in its centre there is now a pellucid point."

The thick transparent membrane of the ovum (or *zona pellucida*) begins to imbibe fluid and distend, so that a minute space filled with fluid is visible between it and the yelk-ball. " This change follows the incipient thickening of the proper membrane of the yelk, and, in some instances, is not appreciable until after the ovum has made its exit from the ovary."

The vesicles or granules of the *tunica granulosa* now appear to hang less tenaciously together, and frequently appear to be passing into a fluid state.

The *retinacula*, at the period now before us, become enlarged, for which there seems to be a provision in their wrinkled state, *ante coitum.*

Most of the changes, says Dr Barry, above-mentioned, have been observed to take place before the ovum leaves the ovary. Some of the later ones, however, are not observed, until the ovum enters the Fallopian tube, so that our author does not take upon him to describe any particular uniform condition in which the ovum leaves the ovary.

This is just as we would expect, when we consider that the numerous changes which we have just recorded all happen in nine or ten hours, and that the precise time at which the ovum leaves the ovary is not always the same in the same kind of animal.

Such remarkable alterations as those we have just described, suggest the idea of sudden and important changes having been effected in the condition of the ovum; that, from the moment that coitus takes place, the germinal vesicle passes from the periphery to the centre of the yelk; a new membrane or vesicle arises from the germinal spot, and enlarging, applies itself to the inner surface of the germinal vesicle. The germinal spot itself becomes transparent in its centre, and also passes to the centre of the germinal vesicle. What, then, is the cause of these remarkable changes? Fecundation, and, from the observations recorded, it is obvious that this must take place in the ovary.

Dr Barry has twice found spermatazoa on the ovary. Professor Bischoff of Heidelberg has also observed them on the ovary of the dog, and Dr Barry has once observed what he imagined to be a spermatazoon within the ovum. Should this last observation be found correct, it clearly and most unequivocally demonstrates to us, that the seminal animalcules penetrate along the Fallopian tubes to the surface of the ovaries, and then pass through their membranous envelopes till they get within the Graafian vesicle. We are inclined to think, however, that this last observation of Dr Barry's may have been fallacious, as the object he has represented as part of a spermatazoon has to us more the appearance of one or more of the peculiar vesicles of the germinal vesicle than a seminal animalcule. At all events, Dr Barry has demonstrated that certain changes take place on the ovum *post coitum*, while still placed within the Graafian vesicle; that spermatazoa penetrate as far as the surface of the ovary, where they have been found at various periods after coitus. We think it likely that here they stop; that their influence alone on the fluid of the Graafian vesicle, or on the ovum itself, through the membranous envelopes of the ovary, should be sufficient to bring about these changes in the ovum, which we have demonstrated to be the result of impregnation; and that, if spermatazoa were essentially necessitated to pass into the ovum, before impregnation could be effected, that they would have been found oftener than once, (this is allowing that it was a spermatazoon that Dr Barry found), considering the number, and great minuteness of the parts, laid open by Dr Barry's researches. We shall, however, expect to hear more of this from Dr Barry. He is still going on with his researches, and we strenuously recommend this point to his notice, as it is one of the most interesting with which we could be made acquainted. We know that spermatazoa are necessary to fecundation; that they pass up through the uterus and

Fallopian tubes to the ovaries, so far is the chain complete on this side. On the other, we have traced the ovum in its immature and mature state; the effects produced upon it by impregnation; the period after coitus that these are first observed, and the period that it leaves the ovary; the middle links of the chain are only awanting, viz. those which will demonstrate to us how soon the spermatic animalcules reach the surface of the ovary, and whether or not they really pass into it.

Changes which take place in the ovum after it has left the ovary.—The changes which take place in the ovum from the period of its leaving the ovary till the incipient appearance of the vertebræ, Dr Barry has divided into twenty-one stages. It will be unnecessary to follow the author into all the minutiæ of these successive stages, as it would be tiresome and too minute for the general reader. We shall therefore give a condensed account of them. The ovum in its passage through the Fallopian tube undergoes the following changes, viz.

1*st*, "An outer membrane, the chorion becomes visible. 2*d*, The membrane originally investing the yelk, which had suddenly thickened, disappears by liquefaction; so that the yelk is now surrounded by the thick, transparent membrane (*zona pellucida*) of the ovarian ovum. 3*d*, In the centre of the yelk there arises several very large and exceedingly transparent vesicles. These disappear, and are succeeded by a smaller and more numerous set; several sets thus successively come into view, the vesicles of each succeeding set being more numerous and smaller than the last, until a mulberry like structure has been produced, which occupies the centre of the ovum. Each of the vesicles of which the surface of the mulberry-like structure is composed contains a colourless and pellucid nucleus; and each nucleus presents a nucleolus."

"In the uterus, a layer of vesicles of the same kind as those of the last and smallest set here mentioned, makes its appearance on the whole of the inner surface of the membrane which now invests the yelk. The mulberry-like structure then passes from the centre of the yelk to a certain part of that layer, (the vesicles of the latter coalescing with those of the former, when the two sets are in contact to form a membrane, the future amnion), and the interior of the mulberry-like structure is now seen to be occupied by a larger vesicle containing a fluid and dark granules. In the centre of the fluid of this vesicle is a spherical body, composed of a substance having a finely granulous appearance, and containing a cavity filled with a colourless and pellucid fluid. This hollow spherical body seems to be the true germ. The vesicle containing it disappears, and in its place is seen an elliptical depression filled with a pellucid fluid. In the centre of this depression is the germ, still presenting the appearance of a hollow sphere.

"The germ separates into a central and a peripheral portion, both of which, at first appearing granulous, are subsequently found to consist of vesicles. The central portion of the germ occupies the situation of the future brain, and soon presents a pointed process. This process becomes a hollow tube, exhibiting an enlargement at its caudal extremity, which indicates the situation of the future *sinus rhomboidalis.*"[*]

Dr Barry has made a most interesting observation, where he found the mulberry-like structure in the interior of the ovum, have a rotatory motion. A similar motion has been observed in the embryos of mollusca, &c. and are no doubt due to the ciliae. It certainly will be curious if future observation will point out a similar rotatory motion in the impregnated ova of mammalia generally. It is the object of the third series of these memoirs to describe minutely the changes which take place on the mammiferous ovum consequent on fecundation, as seen by the aid of powerful lenses; and to make known a remarkable process of development, thus discovered, *by cells*. Dr Barry's observations were made throughout on the rabbit, so that the results might be the more accurate.

It may be premised that Schwann, basing his researches in the animal upon the discoveries of Schleiden in the vegetable kingdom, had demonstrated that in development the same phenomena are exhibited in both; that animal tissues in general, like those of plants, are reducible to modifications of vesicles or cells; and that the mode of origin of the cells is essentially the same in animals as Schleiden had discovered it to be in plants. Dr Barry's observations, then, upon this fundamental principle, may be stated to be as follows. The germinal vesicle is the essential portion of the ovum. The germinal vesicle becomes filled with cells, and these again become filled with the foundation of other cells, and the vesicle is rendered opaque. The germinal spot always presents at a certain period after impregnation a dark point in its centre. This point is soon found to contain a cavity filled with pellucid fluid. The free portion of the spot resolves itself into cells, and the foundation of other cells come into view in its anterior, arranged in layers around the central cavity. "Every other nucleus met with in these researches, has seemed to be the seat of changes essentially the same. The germinal spot, therefore, is the point of fecundation. In proof of which there arise at this part two cells, which constitute the foundation of the new being." Each of the succeeding twin cells presents a nucleus, which, having first passed to the centre of its cell, resolves itself into cells in the manner above described. "By this means the twin cells in their turn became filled with other cells. Only two of these in each twin cell being destined to continue, the others, as well as the membrane of each parent cell, disappear by liquefaction, when four cells remain. These four produce eight, and so on, until the germ consists of a mulberry-like object, the cells of which do not admit of being counted." The same process as that described is followed up in every minute cell. "Every cell, however minute, if its interior can be discerned, is found filled with the foundations of new cells, into which its nucleus has been resolved."*

---

[*] Fig. 4 to Fig. 15, represent the principal changes which take place in the development of the ovum, and which correspond to the 1st, 2d, 4th, 6th, 9th, 11th, 13th, 14th, 15th, 16th, 18th, and last stage (21) of development of Dr Barry; *cho.* (chorion); *b.* (germinal spot); *bb.* (germ); bb^1. (central position of the germ); bb^2. (peripheral portion of the germ); *c.* germinal vesicle; *d.* yelk, yelk globules; *e.* membrane of yelk; *f. zona pellucida.*

The process above described, viz. the origin and increase by cells in the mammiferous ovum, is no doubt universal. There appears to be evidence of its occurrence in the ova of batrachian reptiles, some osseous fishes, and certain of the mollusca.

The Chorion.—Dr Barry has shown, with regard to this membrane, that it is not the thick transparent membrane (*zona pellucida*) of the ovarian ovum, but a thin lamina which rises from the surface of that membrane. That this is the case has been proved by tracing it from its origin in the Fallopian tube up to the period when it becomes villous in the uterus. The chorion has now been proved also to have its origin in cells. These are not the cells or granules of the *tunica granulosa* brought with it from the ovary, but cells which arise on its surface, and interlacing with each other, and growing by addition of cells, form the incipient chorion. Dr Barry promises us a more minute account of the cells which give origin to the chorion, in a future paper. We shall reserve any further remarks which we may have to offer on this head till then.

Our space will not allow us to enter more fully into the examination of this most interesting subject. We, however, hope soon to have more of Dr Barry's researches, and that, as he has taken the lead in those investigations, he will keep it until he has thoroughly and satisfactorily explained every minute link which may be awanting, in the grand chain which he has so nearly, already made out. We wish him every success, and, as he has perseverance, so we are sure has he talents for the task, and that we shall soon be able to lay before our readers other novelties from the pen of Dr Barry, in this path.

Before concluding, however, we must say a word or two regarding the beauty and fidelity of the representations which accompany the work. Any one who has been accustomed to see *ova* through a powerful microscope, be they fecundated or not, must have been struck with the fidelity of the present series of delineations; and they are so accurate and minute that many of them, to be seen to advantage, ought to be examined with a lens. They are from the pencil of Dr Barry, engraved by J. Basire. A few of the most important and essential of these we have copied and introduced at the proper place, in order to illustrate the leading facts of the inquiry. This, however, we must add, that all that we can pretend to do is merely to give a sort of specimen of the labours of Dr Barry. In order to form a just estimate of the difficulty of these labours, the fidelity with which Dr Barry has studied to illustrate his inquiry, and the importance of the facts which he has brought to light, it is quite indispensable that the reader should study these papers with the greatest care. He will then be in a condition not only to judge of the difficulty of the task, but of the great merit of the author.

---

* Figs. 186 to 231 inclusive, represent the changes which take place in the cells of the germinal spot and vesicle. *b.* germinal spot; *bs.* twin cells, foundation of the new being, germ, mulberry-like object; *bb.* rudimental embryo; *c.* germinal vesicle; *e.* proper membrane of the substance by which the germinal vesicle is surrounded; . thick transparent membrane of the ovum; *zona pel.ucida*; *cho.* incipient chorion.

ART. II.—*Traité Pratique d'Auscultation, ou exposé méthodique des diverses applications de ce mode d'examen à l'etat physiologique et morbide de l'economie.* Par M. BARTH, Docteur en Medecine, &c., et M. HENRI ROGER, Docteur en Medecine, &c. Paris, 1841. 12mo. Pp. 510.

SCARCELY had Laennec published to the world his admirable observations on diseases of the chest, than auscultation was received as one of the happiest discoveries of modern times, and was adopted by many of the ornaments of the science of medicine in France, England, and Germany, who increased the utility of this method of diagnosis, by extending its application to the practice of surgery, and demonstrating its utility as an important sign in the diagnosis of pregnancy.

The present work is from the pen of Drs Barth and Roger, physicians distinguished for the facilities they have been in the habit of affording to the English student for the study of the stethoscope, and who have brought their long experience, as Internal or Clinical Resident pupils in the hospitals of Paris, to bear upon this very important and interesting subject.

The authors of this well-arranged manual have endeavoured, and we consider successfully, to present us with the science of auscultation as it at present is, and have accordingly united the facts and precepts laid down by Laennec with the discoveries of modern times throughout Europe.

After a short history of auscultation, along with remarks on certain rules to be observed in the employment of this method of diagnosis, they enter more exactly upon their subject. The work is divided into four sections, including auscultation of the chest, auscultation of the abdomen, auscultation of the head, and auscultation of the limbs. Drs Barth and Roger commence by examining the respiratory murmur in regard to alteration of intensity, of rhythm, of character, and in reference to abnormal sounds; and after an investigation of the changes which the voice undergoes in certain conditions of the lungs, they give a very interesting and important chapter upon the auscultation of the larynx, where, profiting by the valuable researches of Dr Stokes on this subject, they add some useful practical remarks.

This manual has the rare merit of simplicity, and the authors appear to have restrained, rather than augmented the nomenclature of auscultation; and have avoided minute subdivisions which, when carried too far, as is occasionally done, only obscure and complicate a subject, instead of rendering it clear and easy of comprehension.

Drs Barth and Roger have taken a kind of middle course in their investigation of the causes of the normal sounds of the heart, and confess themselves to have been much aided in their inquiries by the Report of the Dublin Committee of the British Association.

The sections upon the auscultation of the head, abdomen, and limbs are necessarily short, in consequence of their smaller importance. The importance of auscultation, as a means of diagnosis in pregnancy, is discussed in an appendix, which appears to us to contain the most complete and exact information which we possess on this interesting subject.

The admirable manner in which the recapitulation at the termination of the work is composed, is well worthy of imitation, and must be considered a very useful appendix to this valuable little volume.

ART. III.—*Vital Statistics of Scarborough.* By JOHN DUNN, Surgeon. 4to. London, 1840. Pp. 11, and a Series of Four Tables.

THE importance of statistical researches is becoming every day better appreciated; but there is one element which it is absolutely necessary these should possess, and without which they are entirely useless, and that is, that the statements on which the calculations are made should be founded on well-ascertained facts. It is to be feared, however, that in Mr Dunn's statistical researches for Scarborough, an error occurs in the basis on which his calculations are founded, which, to a certain extent, lessens the value of what in other respects is a most interesting document.

No documents being accessible, by which the actual increase of the inhabitants of Scarborough, since the last census, can be obtained, and not being aware of the late changes which have taken place there, it is impossible to ascertain how far Mr Dunn's statements are correct. But, taking it for granted that the population of the town has gone on increasing since 1831, in the same proportion relatively to the excess of its births over its burials, as it did from 1821 to 1831, it will at once be apparent, that Mr Dunn's assumed estimate of the increase of the population is too high. For instance, he mentions that, from an average of the last three years, "an increase of about 69 births takes place over the deaths of every year; and, multiplying this number by 9, the last census being made in 1831, adds 621 to our population." Such, however, is by no means a correct mode of estimating the increase of the population of a sea-port town like Scarborough. The census of 1821 proved the population to consist of 8533 persons, that of 1831 of 8760, showing in ten years an increase of 227 persons, or 22 per annum. But, according to the parish register abstracts, published alongst with the parliamentary census of 1831, for the same period of time, there was an excess of 410 births over the burials, which ought to have given an annual increase to the population of 44 persons, or double what was actually found to occur.

Mr Dunn's average of the annual number of births, drawn from an observation of only three years, is not likely to be more correct than that above-mentioned; three years being far too short a period to admit of drawing correct averages in a limited population. To be satisfied of this, the following facts may be mentioned. During the three years, 1825–26–27, the excess of births over the burials in Scarborough and its suburbs was only 9 per annum; whilst, for the very next three years, 1828–29, and 30, the annual excess was 85; and if Mr Dunn's observations as to the excess of births for another triennial period, viz. 69 per annum, be added, it will at once be apparent how fallacious any conclusions must be which are based on such a short period of observation.

Scarborough does not stand alone in the singular fact of the excess of its births over its burials greatly exceeding the actual increase of its population. The whole North Riding of Yorkshire is in the very same situation. Thus, between the years 1821 and 1831, the population of the North Riding, as indicated by the excess of the number of births over that of the deaths, ought to have increased by 24,674 persons; but the actual increase, as indicated by the parliamentary census, was only 3304, leaving the immense number of 21,370 individuals unaccounted for. Since 1801, the same difference between the actual increase of the population, and that indicated by the excess of births over deaths, has been observed in the same place. Thus, between 1801 and 1811, there were 8315 births which failed to be accounted for by the actual increase of the population during that period; and from 1811 to 1821, the difference amounted to 4983.* This is a most curious fact, and one which must tend to invalidate all attempts at accuracy in local statistical rêsearches within the district where such an occurrence takes place.

---

* Abstract, &c. of the population of Great Britain, 3 vols. folio, Vol. iii. p. 399. *London*, 1833.

In endeavouring, then, to arrive at a correct estimate of the actual increase of the population of Scarborough since the last census, as it has been shown that the plan taken by Mr Dunn for this purpose is founded on error, let it be taken for granted that the population has just gone on increasing in the same ratio as it did during the ten preceding years, *i. e.* from 1821 to 1831, during which period there was an annual addition to the population of 22 persons. This would cause the population of Scarborough in 1840 to amount to 8958, instead of 9500, on which number Mr Dunn's calculations are founded.

So much for the increase of the population. It may be now useful to inquire whether Mr Dunn's statement of the mortality of this place, from a three-years' observation, gives a better or fairer average of the comparative salubrity of the climate, than his statement of the annual number of births did regarding the increase of the population.

From July 1837 to July 1840, Mr Dunn states that 733 deaths occurred. From the Parish Règister Abstract, we gather that, for the triennial period 1822–23–24, the mortality in Scarborough and its suburbs amounted to 513; for 1825–26–27, to 581; and for 1828–29–30, to 468, showing a greatly diminished mortality to what has been observed by Mr Dunn in his three years of observation; a circumstance which tends to prove the climate there to be more salubrious than even Mr Dunn's most sanguine expectations could have led him to hope.

Mr Dunn's observations extend from 1st July 1837 to 1st July 1840, including a period of three years; and he not only gives tables of the mortality during the different months of these years, and of the various diseases which caused this mortality, but also tables of the weather, including the monthly maximum, minimum, and average height of the thermometer and barometer, the number of rainy and fair days, the prevailing winds, &c., and he also compares these with the actual monthly mortality. A fourth table is added, "illustrative of the climate of Scarborough for eight years," giving the maximum and minimum range of the thermometer for that period.

The number of births during the three years of observation of Mr Dunn was 939, 513 being males, and 426 females. The number of deaths for the same period amounted to 733, of which number 346 were males, and 387 females. It is important to notice the large proportion of females in this town. At the last census of the population, taken in 1831, there were only 3704 males, whilst the females amounted to no fewer than 5056; which accounts for the great proportion of deaths amongst the females at Scarborough.

The classification of diseases which Mr Dunn has adopted is founded on that of the Registrar-General, from which it differs in a very few unimportant points; but on which it is unnecessary to remark here, as our views were fully detailed in the last number of this Journal. In addition to giving the general results for the three years of his observation, he has extended his tables, so as to show the actual mortality of the different diseases for every month of that period.

The mortality for the three years being 733, the following is the number of deaths caused by each class of diseases:—Epidemic, endemic, and contagious diseases proved fatal to 160; diseases of the nervous system to 176; those of the respiratory organs to 144; those of the organs of the circulation to 11; those of the abdominal organs to 41; those of the urinary organs to 5; those of the generative organs to 4; disease of the cutaneous system to 1; diseases of uncertain seat to 102; old age to 77; violent deaths to 6; and causes not specified to 8. More than one-half of the mortality, or 447 deaths, occurred under nine years of age, and of this number 197 were under one year. The deaths above eighty years of age amounted to 93.

The influence of the seasons on the mortality shows, that, for the months of July, August, and September, the mortality was lowest, being only 149 for the three years; for the months of October, November, and December, the number of deaths was only 150; for April, May, and June, the mortality rose to 195; but was much greatest for the months of January, February, and March, amounting to 239. This result differs considerably from what occurs in the other part of Yorkshire, or in its North Riding, in which Scarborough is situated; for over all this district, the greatest mortality occurs in the months of April, May, and June; but it corresponds with the general result of the mortality over England and Wales,—January, February, and March being the months during which the greatest amount of deaths take place.

But little reliance can be placed on any deductions drawn from the small amount of diseases occurring in so limited a population as that of Scarborough. This is especially the case with regard to the epidemic, endemic, and contagious diseases, and is well exemplified in the statistics for the three years observed by Mr Dunn. Thus, in the year ending 1st July 1838, the mortality from these diseases amounted to 81, in the succeeding year only to 19, and in 1840 to 60. The diseases of the respiratory organs proved most fatal in the month of February; then in order in January, June, and July; and were least in May. These diseases bore the proportion to the general mortality of only 1 in 5.09, whilst, over England and Wales, the proportion appears to be 1 in 3.67. Pulmonary consumption proved most fatal in the month of July,—a fact which the late Dr John Home pointed out to occur amongst the patients admitted to the Royal Infirmary of Edinburgh.*

The number of deaths from other affections is too trifling to call for special notice. One circumstance, however, appears to deserve notice, and that is the large amount of aged individuals in this town, no fewer than 93 of the deaths being in individuals above 80 years of age. The same remark is applicable, with the exception of the mining districts, to all the North Riding of Yorkshire, which, indeed, with Norfolk and Suffolk, appear to be the most favoured districts in the kingdom, so far as regards the probabilities of life and exemption from disease.

The thermometric tables show the remarkable mildness and uniformity of the climate at Scarborough. One of these tables is an abstract from a register of the weather, kept for the last eight years—a period sufficiently long to allow of a pretty correct estimate being formed of the climate there. During the first year of Mr Dunn's observations, a much greater diurnal variation or range of the thermometer was observed than during the two subsequent years, and it was during this year that the greatest amount of deaths occurred, viz. 296; the mortality for the second year of observation being 205; and for the third 232; each year the mortality bearing apparently a close connection with the sudden alternations of temperature. The mean daily range, however, of the thermometer for the last three years was only 8.93 degrees.

The barometric pressure of the atmosphere was observed to be remarkably uniform, the maximum height of the mercurial column never having been above 30.97 inches, nor its minimum below 28.22; the mean point during the whole of that period varying merely from 29.60 to 29.89.

During the three years of observation the winds blew 612 days from a westerly direction; 367 from an easterly direction; 137 from the north, and 122 days from the south.

The mean annual fall of rain for the last eight years was only 22.10 inches; but the fall during the years 1836–37–38–39 was nearly one-fourth greater than during the four preceding years.

ART. IV.—1. *Observations on the Oriental Plague, and on Quarantines, as a means of arresting its Progress.* Addressed to the British Association of Science, assembled at Newcastle, August 1838. By JOHN BOWRING. Edinburgh, 1838. 8vo. Pp. 45.

---

* Edinburgh Medical and Surgical Journal, Vol. xlix. p. 1, 1838.

2. *Quarantine Laws, and their Abuses and Inconsistencies.* A
Letter addressed to the Right Hon. Sir JOHN C. HOBHOUSE,
Bart. M. P., President of the Board of Control. By ARTHUR
T. HOLROYD, Esq. London and Edin. 1839. 8vo. Pp. 65.

AMONG the questions which ever and anon agitate the minds and divide the opinions of the medical profession, that of infection and contagion holds a prominent place; and, from the influence which it exercises on the commercial intercourse of remote countries, its discussion will always afford more or less interest, not to members of the profession only, but to a large and important body of the community, who are connected with or concerned in commerce, trade, or manufactures. The question of the contagious nature of such diseases as yellow fever, cholera, and plague, cannot fail to afford fertile subjects of interest to any mercantile country, so long as it is believed, or even asserted, that it is possible for the infectious principles of either of these distempers to be imported into the country in any of those multiplied articles which are continually brought from the places where they prevail endemially. The question of the contagious nature of yellow fever has been so long, so keenly, and so ably contested, that, if it be not now decided, it may safely be said that it never will. All the facts necessary to the determination of the question, whether that distemper be in its nature essentially contagious or not, may be said to have been long collected, ascertained, and placed before the profession. If from these it cannot yet be decided whether the disease be contagious or not, it seems difficult to understand what other evidence can be imagined to be requisite. To get rid of the question of essential or necessary contagion, and, in consequence of the inadequacy of the facts adduced to establish this doctrine, some speculators imagined another variety of contagion, that, namely, which has for many years been known by the name of contingent, accessory, or conditional contagion. By this doctrine, it is understood that a distemper which is not in itself or essentially infectious or contagious, may, under certain circumstances, acquire this property. This doctrine, which was originally thrown out as to plague by an anonymous writer in 1751, and as to yellow fever, and some other distempers in 1763, by Dr James Lind, has, since the commencement of the present century, acquired considerable importance, from the circumstance of its having obtained, in a slight degree, the sanction of the name of Baron Von Humboldt, and, if we understand him right, Sir William Burnett; and, certainly, from the convenient manner in which it admits of application to several different sorts of fever, it is well calculated to procure partizans.

The true plague of the Levant, the anthracoid, carbuncular, or glandular plague, is a distemper which has been very generally regarded by physicians as essentially, at all times, and in all circumstances, decidedly and unequivocally contagious. Whether it has been owing to the extremely rapid progress of this disorder, or to its dreadful ravages and great mortality, or to the little control exercised over it when introduced in any community by merely medical measures, it is certain that it has been very generally presumed to be highly contagious; and many have gone so far in this way of thinking, that they have never even doubted the contagious character of the disorder. By a very small proportion only of physicians has the opposite doctrine been avowed and taught; and so strong and irresistible has been the current of medical opinion on the contagious properties of the plague, that very few have had the boldness to maintain and defend the idea that it is not contagious; and those who have done so, have apparently been overwhelmed and lost amidst the multitudes who have repeated the generally received doctrine.

It is not improbable that it is chiefly owing to this overbearing force of the voices of the majority, that the number of those who have ventured to call in question the contagious origin and propagation of plague, is very small compared with those who have maintained that it is a contagious distemper. There have not been wanting, however, at all periods of the history of medicine, ingenious, and sometimes original writers, who have ventured to doubt the correctness of the ordinary doctrine; and, while several of these seem to describe plague, as if contagion was a character never to be thought of, others have taken some pains to show that the adjunct of contagion was, if not wholly imaginary and unfounded, at least in a great measure gratuitous. It may be not unseasonable to advert to the facts, and observations, and arguments of some of those writers, as introductory to what we are here to say on the tracts of Dr Bowring and Mr Holroyd.

When the distemper which has since been ofttimes seen and described as glandularplague, first broke out in the Levant in the reign of Justinian, in the year 541, it seems to have been very generally believed that it was contagious in origin and propagation. Of this notion, however, no very decisive proof has ever been adduced. A statement, that it broke out in Ethiopia, according to some, and in Egypt, according to other authorities, shows only that it was not ascertained what was the original locality in which it first appeared. Since the days of Thucydides, plague has always been said to have been engendered in, and to have issued from Ethiopia; yet it is remarkable that we have no authentic proofs that genuine glandular or carbuncular plague was ever known in that country.

With regard to Egypt as a parent of pestilential distempers, it is well known, that, for several centuries, the cities and towns of

Lower Egypt have been from time to time afflicted with plague, which the inhabitants invariably assert proceeds from Barbary, Syria, and Constantinople; while, on the other hand, when plague appears in Constantinople, Smyrna, or Aleppo, or on the Barbary coast, it is quite as confidently believed by the inhabitants of these places to proceed from Egypt.

It is very properly observed by Dr Arbuthnot, that all that can be inferred from this is, "that there is something infamous as well as terrible in the plague, so that no nation will own it to be natural to their country."*

One of the earliest good authorities upon the subject of the plague, as it appears in Egypt, is Prosper Alpinus, an eminent Italian physician, who went to Grand Cairo in September 1580, and, after remaining there rather more than three years, returned to Venice in 1584. During this period he collected much information on the diseases proper to Egypt, and the practice pursued by the Egyptian physicians, and especially on the circumstances under which the plague is observed most usually to appear and to disappear. Although assuming throughout the principle that the plague is a contagious disorder, yet he is led to mention several circumstances which are rather inconsistent with this idea.

Thus he states that the plague appears in Cairo and all the other towns of lower Egypt very regularly at the commencement of the month of September, and continues to prevail till the month of June, during which it as regularly disappears soon after St John's day, viz. the 12th or 17th, O. S., that is 24th June, N. S. He states that this disappearance of the disease, which corresponds very accurately to the time when the sun enters Cancer, is so sudden and complete, that it seems almost miraculous or of divine origin.

Prosper Alpinus further adds, that so completely does the contagion appear to be extinguished at the period now mentioned, that articles of furniture, bedding, clothing, and similar objects which had been used by the sick, are then currently used by the sound, without giving rise to the distemper, and without the apprehension of their doing so.

The two facts now mentioned must be allowed to be rather at variance with the idea of contagion. At least, it is a curious sort of contagious principle which commences so regularly at one period, and terminates with equal regularity at another. At the same time, it would not be quite philosophical to deny the existence of contagion on the grounds now specified. These facts merely show that it is not very probable; but they are not sufficient to show that the existence and influence of contagion is impossible.

---

* An essay concerning the Effects of air on human bodies. By John Arbuthnot, M. D. Lond. 1751.

In considering the circumstances which render the month of June different from any other month in the year, Alpinus observes, that, excepting the rise of the Nile, which takes place at this time, there is nothing by which this season is distinguished from any other; and he afterwards mentions as two other circumstances, the entrance of the sun into the tropic of Cancer, and the blowing of the Etesian or annual northerly winds. Regarding the first cause, he maintains that the very small increase of the Nile which takes place during the first days of its rise, however regular, is totally inadequate to account for the sudden disappearance of the epidemic distemper; because he argues the air cannot thereby be so altered from its previous state, and purified, as to rectify its constitution and remove the disease.

With regard to the Etesian or annual northern winds, he thinks their influence may be considerable in seasons and countries where they are regular in their currents; and they appear to be more regular in Lower Egypt than in most other countries. It is also matter of observation, that, whenever these north winds blow regularly and steadily, the warm season is invariably less sickly. But, he observes again, that there are other countries in which the Etesian winds blow steadily, yet in which their breezes do not appear to have the power of entirely extinguishing the disease. This was particularly observed in the plague which prevailed at Padua in the year 1575.

The entrance of the sun into the tropic of Cancer is so closely connected with the blowing of the Etesian winds and the swelling of the Nile, that it may be regarded, so far as its influence on the state of the health is concerned, as quite the same.

The work of Prosper Alpinus appears to have been published at Venice in 1591; and, both about this time and subsequent to it, many authors wrote on the nature and the causes of pestilence, and gave directions for protecting against its attacks. Of these very few adopted the opinion, that it was wholly a contagious distemper; and the majority seem to have regarded it as the result of some morbid or distempered state of the atmosphere. At the same time, many of them admit the influence of contiguity, contact, and contagion, or rather a poison which operates by contact. In the seventeenth century, these ideas were so general and so much matured, that the disease was commonly denominated the Contagion by eminence; and in this mode Cellino Pinto, who published a treatise in 1631,* and Francesco Rondinelli, who wrote in 1634† on the Florentine plague of 1630, speaks of this distemper.

---

* Compendioso Trattato Sopra l'Male della Peste e Contagio, sua preservazione e cura. Opere utile e necessaria publicata a beneficio commune da Cellino Pinto, Medico. in Bracciano, 1631, 12mo.

† Relazione del Contagio stato in Firenze l'Anno 1630—33. In Fiorenza, 1634, 4to.

It would lead us into too much detail to examine the opinions and doctrines of even a small proportion of these writers. All of them admit the old doctrine of the contamination of the air, and sometimes the waters, and even the soil; and several, besides, allow the influence of a poison operating by passing, as they suppose, from one to another. Of this, however, they give no proof; and much of their arguments consist in fanciful speculations, with few or no facts.

When the plague appeared at Marseilles in 1721, it called into the field a considerable number of writers; and among others Sir Richard Blackmore and Mr Soame. The former, without espousing the non-contagious side of the question, maintained, however, the origin of the plague from various morbid states in the air, and from other conditions in different countries. The latter confined himself very much to a relation or narrative of the plague in Marseilles, which, however, he inferred, owed its propagation to contagion.

The next author of any note, who has directed his attention to the etiology of plague, is Dr John Arbuthnot, who, in an excellent essay on the effects of air on human bodies, published in 1751, endeavoured to explain the manner in which plague and various similar disorders are engendered. In this essay he appeals to the authority of Prosper Alpinus, and various other authors, to show that the air is liable to become disordered in various modes, and that this disorder may be sufficient to generate various pestilential disorders.

Dr Arbuthnot then expresses an opinion, which, if it have any meaning whatever, implies that plague is the native product of the Egyptian soil; the offspring of the physical causes operating there; and, consequently, inconsistent with the idea that it is either contagious or imported.

" Pestilential distempers," says Dr Arbuthnot, " are frequent in Egypt. * * * But what I think demonstrates the plague to be endemial in Egypt, is its regular invasion and going off at certain seasons, beginning about September, the time of the subsidence of the Nile, and ending in June, the time of the inundation. In the first case are all the causes productive of putrefaction, heat, and putrid exhalations, and no winter frost to check them. But what is wonderful, the plague and the fevers, from the heat of the Campsin, go off by the northerly winds and the overflowing of the Nile. The wholesome quality of the northerly winds for checking pestilential distempers has been observed by all ancient physicians; and to show that the plague depends upon the temperature of the air, Prosper Alpinus takes notice that, upon the swelling of the Nile, the infection, and even the danger from infected clothes and furniture, goes off; besides the cooling of the air, the northerly winds may dissipate the stagnating vapours, and the running of the Nile the stagnating water. Galen and Hippocrates both observed that the Etesian or northerly winds blowing in summer made a wholesome season."

The facts now specified are rather opposed to the notion that plague depends either in origin or in propagation on a contagious principle. Indeed, everything said both by Prosper Alpinus and Dr Arbuthnot tends to show that it is endemial in Egypt or the product of the soil and climate. That it may afterwards, under peculiar circumstances, acquire contagious properties, is quite possible, and may be the case; but this admission is totally inconsistent with that of the disease being essentially, originally, and at all times of a contagious nature.

The same year in which Dr Arbuthnot published his Essay on the effects of air on human bodies, a very strenuous advocate for the plague being void of contagion, appeared in the work of an anonymous author.* This writer has not only adduced all the arguments usually brought forward both by Arbuthnot and others to prove that plague depends rather upon the state of the air, than upon any contagious principle; but he has anticipated Dr Maclean in showing, whether correctly or not we do not now say, that the doctrine of contagion was not originally or primarily maintained by physicians; but only got a footing among the dogmata of the public authorities, and afterwards the medical bodies in the middle of the sixteenth century, (1547), when it was espoused by Pope Paul the Third, for political objects, viz. to get the General Council of the Church which was to be held at Trent, translated from that city, under the pretence that it was ravaged by a pestilential disease, to Bologna. It must, indeed, have been either from this essay, or another of the same kind, by Mr Dale Ingram, to be mentioned presently, that Mr Maclean obtained his information; for both of these writers maintain with him that plague was not taught to be a contagious disease till some time after the meeting of the Council of Trent in 1547.

The anonymous is one of the first of modern writers who distinguishes between infection and contagion;—understanding by the former a particular state of the air favourable to the formation of disease, and actually capable of producing disease; and, on the other, meaning by contagion that principle or disease, by virtue of which it passes from one thing or one person to another.

"A plague," he says, "is really the product of every country; because the two memorable constitutions of air and breeders of plagues are everywhere to be met with; and the fore-mentioned constitutions of air are certainly capable of infecting every person within the sphere of its activity, they being equally and duly disposed." We find, then, that he adopts the doctrine of contingent or accessory contagion. "In that case," says he, "the efficacy of the air is not scanty, and able to infect only two or three people; in order to their transferring it by contagion, and propagating the plague to the rest. Which notion is by no means philosophical, and impeaches the power of the state of the air against all experience."*

---

* A Treatise on the Plague and Pestilential Fevers, with some useful hints for the better prevention and cure, together with some observations on the pestilential fever, now raging among the horned cattle; and the reasons for the necessity of rectifying the present ill state of physic in this nation. London, 1751. 8vo. Pp. 84.

The following arguments have been often urged against the contagious characters of different diseases and against the doctrine of contagion in general; and perhaps they are more useful in showing that the author doubted or denied the presence of contagion in plague, than that plague is not a contagious distemper.

"In the plague we ask, how is the first man infected? and may not the infection of the plague be received without the concurrence of some things emitted from infected persons. Yes, sure, from unwholesome air. And it may be demonstrated from the nature of the plague, that it is never or very rarely communicated from one person to another. And the proof of the fact may be taken from Captain Graunt's Observations on the Plague in 1636, p. 70, (Bills of Mortality;) from the sudden jumps which the plague then made; leaping in one week from 118 to 927, and from thence again the very next week to 852; which effects must surely be rather attributed to a change of the air, than of the constitutions of men's bodies, otherwise, than as the latter depends upon the former; so that the infection of the plague depends more upon the disposition of the air than upon any effluvia from the bodies of men. This is truly a demonstration, and every one who understands the manner of Captain Graunt's reasoning must acquiesce in the truth of it. The sense of it is this; the captain is inquiring whether the steams coming out of our bodies, or the air, are the principal causes of spreading the plague; which he determines by the foregoing argument in favour of the air; for every body knows how changeable the air is; which, by its different changes from better to worse, can speedily make the plague more grievous and general; as, on the other hand, its changes from a worse to a better constitution, can make the plague more gentle and less diffused—and it is these speedy changes of air that are the causes of the jumps we find the plague often makes, while it rages and is doing the greatest mischief, so that infection is never or rarely conveyed from the body of one man into that of another. And the late Dr Radcliffe seemed to be of this opinion; for, on being asked one day how men caught the plague? he readily answered: suppose it should rain, and we two should go together into the rain, we should certainly both be wet, though we did not wet one another. Hence it may be inferred, that it was his opinion that the infection of the plague was communicated from the air, and not from man to man by contact; agreeable to what the great Sanctorius has long since observed, namely, that we are infected with the plague, not by the touch, but by drawing in the pestilent air with our breath. The proofs for this conclusion are, that we do not find any putrefaction or corruption brought into, or made in our blood; and what the blood itself has not in it, cannot possibly be propagated by contagion. Moreover, we find that our steams do not convey any thing, any length, pernicious to any other person, and, consequently, that they seldom, if ever, are the cause of the plague."

---

* A Treatise on the Plague and Pestilential Fevers. London, 1751, p. 19.

He next adduces, in proof of the correctness of the same mode of thinking, the fact related by Dr Howel from Procopius, as to the great plague which prevailed at Constantinople in 543, viz. "that no physician or other caught the disease by touching the sick bodies; and that many continued free, though they tended and buried infected persons;" and the analogous part observed in the plague of Marseilles, that mothers and nurses sick of plague, and even who had died of the distemper, did not communicate the disease to the infants which they suckled.

This argument, we fear, cannot be regarded as very conclusive, since it is always capable of being explained away and nullified by the doctrine of insusceptibility. It is well-known that, in every plague, of the persons fully and sufficiently exposed to the sick, several are not attacked, and also that infants are almost never attacked either by plague or by fever. The author of this tract, nevertheless, thinks, that facts of this kind are so satisfactory and pointed, that they bring us, he says, by mere induction, to this conclusion, namely, that the infection of the plague is rarely, if ever, conveyed from the body of one into that of another person; or that the plague is not a contagious disease.

After thus showing that the plague does not pass from one man to another, he next undertakes to prove that it is not conveyed by merchandize or any other goods.

Of his reasoning employed to establish this point, part is speculative, and part consists of actual facts.

After first observing that neither experience nor history can be adduced in favour of the opinion that the pestilent infection is conveyed by goods, he adds the following remarks:

"To convince us of this, we need only recollect, what has been already shown above, that the air does not produce, far less bring any real corruption into the blood when it produces the plague. As also, that, whatever the state of the blood may be, there is not any corruption carried from the body of the sick into the body of the sound.

"These are principles which we have learned from pure observation; and, consequently, that the plague is not a contagious disease; it being in the nature of a contagious disease, that its infection is known to be communicated from one man into another by the touch. We may therefore wonder how goods receive corruption, and how they more readily convey the plague, than one man does to another. This ought to have been made very plain by observation and experience, the only certain way of coming at the truth in this affair, before such opinions and reports had been currently spread."

These reflections are not very pointed or conclusive, and must be regarded as rather conveying the sentiments and belief of the author, than demonstrating the facts of the case.

He afterwards, however, gives more direct evidence in the following facts.

"The merchants who lived in Grand Cairo during a plague, attended their warehouses when their goods were packed up, and assisted in the business of flax and cotton, without receiving any hurt to themselves, or perceiving any in their labourers. The like accounts we have from Smyrna, Aleppo, Scanderoon, Constantinople, &c; and what is particular, cotton is the merchandize they smell at, and handle more than any other. These merchants sent us goods some years ago from Aleppo, while there raged in it a most destructive pestilence, that carried off 90,000 souls in half-a-year; yet neither plague, nor any degree of it, was perceived here; and the merchants inform us, that there is not any place which is always infested with the plague, not Grand Cairo itself (notwithstanding the ill name it bears), as also, that plagues keep in so constant a course, that they know their duration and violence, by their appearing earlier or later in the spring.

"We have likewise our own experience, and that of our ancestors, for the Turkey goods never having brought the plague amongst us, in upwards of an hundred years. And let these merchants be supposed as partial for their own trade as we can reasonably, yet their testimony is the more unquestionable, because it is no more than what was given for 1900 years before, and is at present the common opinion of Franks, as well as of the Turks and Greeks.

"What greater proof, more ample and particular, can be given of any fact in question, than what we have here given against the mistaken notion that the infection of the plague is conveyed in goods?"

A considerable number of arguments of the same nature, he adduces to the same effect, and at the same time shows the fallacy of the usual opinions on the powers ascribed to goods of communicating the distemper.

The chief object of all these facts and reasonings is to show that the existence of pestilential contagion was never suspected, and that the disease was never imagined to be conveyed in this manner by the civil governors or the most experienced physicians of ancient times; and also to prove that the whole of this doctrine was of recent origin. He then informs us at once that the first person who in modern times made any change in the ancient doctrines, was Fracastorius, of whom he speaks in the following terms.

"Indeed, it must be owned, that he is not only the first author, but the last too, who has treated that subject in a rational manner; but had not this new doctrine fallen in with a very particular conjuncture of politics, it had neither been so favourably received, nor had it subsisted, in all probability, till our times; but as it gave an handle for a law, so princes have, in some degree, kept it in being. It was about the middle of the sixteenth century that this opinion was vented; and the year following, the political pope, Paul the Third, made his use of it; as may be seen in Paul's History of the Council of Trent. Hence, you see the rise and origin of the notion of pestilent infection being capable to be conveyed from one man into another. And this gave the handle for shutting up houses in times of pestilence. And, without doubt, this notion gave the rise to the Act of Parliament in the first year of King James the First, chap. 31. But this act was broke by authority, in the time of the plague in 1665; as soon as its ill effects, and the hurtful consequences of it were discovered."

We see in the extracts now adduced, and the train of reflection pursued, the whole of the train of reasoning which has been adopted by several of those authors who have, subsequent to the time of the one now mentioned, maintained that the plague was a distemper void of contagious properties. Such very nearly was the line of argumentation pursued by Mr Dale Ingram, surgeon and man-midwife, first of Barbadöes, and afterwards of London, who published in 1755, four years after the anonymous tract already noticed, an Inquiry into the opinion, that the Plague is a contagious distemper, capable of being transported in merchandize from one country to another. In this tract, Mr Ingram, after a short account of all the plagues that had prevailed from the great one, of 1346–47,–48 and 49, to the plague of Marseilles, undertakes to prove the absurdity, as he terms it, of the notion, that the plague is contagious, and to refute all the arguments adduced to show that it is contagious. In this inquiry, Mr Ingram does not profess himself a disbeliever in all contagion; but he takes care to inform his readers that he believes several diseases to be contagious, especially small-pox and continued fever. Many of his facts and arguments are taken from Prosper Alpinus, Dr Arbuthnot, and the anonymous author of the tract on plague; but he shows considerable skill in placing them in the strongest and clearest light; and several he has added from personal knowledge and observation. The plague and all similar distempers, he ascribes solely to distemperature, or an unhealthy constitution of the air. To trace this train of reasoning would occupy too much time; but his views may be understood from the following extracts.

" From which it may be concluded, that the indisposition of the atmosphere is the original cause of this disease; and if intrenchments, lines, fortifications, quarantines can stop the current of the air, certainly they ought without loss of time to be put in execution. But I am afraid all will be ineffectual, unless we throw up great hills and mountains, to assist all the other contrivances."*

With regard to those most nearly in communication with the sick:—

" Let us particularly consider the condition of physicians, apothecaries, surgeons, and nurses. The question is, then, whether more of these people in the plague of London did escape with life, or died? According to what I have read, scarce any of them died, which shows that the cause of the plague spreading was more from the air than contact."†

As to fomites, or articles supposed to be impregnated with the contagious poison of plague, the following extract may be taken:—

" Every one who has been in Turkey well knows that Stevidores and sailors often work naked in stoving the cotton. This is a laborious employment, and the hold of the ship is excessive hot. Further, the cotton packs or bales are continually handled and tumbled about, to fix the pieces of wood on, to facilitate their storage. The labourers, too, are often bare-footed, therefore they cannot help treading continually upon the merchandize brought aboard. Nay, I have seen the sailors breakfast, and others sleep on these bales of cotton, &c.

" Surely, then, if the seeds of contagion were lodged in cotton, these men could not escape."

In short, Mr Ingram maintains decidedly, that no evidence had been adduced in his time, sufficient to prove that plague was always a contagious distemper, or that it was conveyed either from Turkey or Egypt by means of contagion.

Subsequent to the time of Mr Ingram, similar notions were entertained by various authors. Stoll of Vienna, (1777), in particular, regarded plague as merely an intense or malignant form of fever; and as he did not consider the latter contagious, the former he also regarded as void of contagion. Several of the physicians who accompanied the French army into Egypt in 1799 and 1800, appear to have considered plague as endemial in that country, as an indigenous distemper of the soil, the climate and the habits of the natives, and, consequently, as a distemper not necessarily contagious. This appears to have been the opinion of Savaresi, Larrey, and Assalini. Pugnet proposed the question, whether the plague is endemial in Egypt, and whether it was possible to eradicate it from the country? The first question he answers,—after a long train of pointless and inconclusive reasoning, in the affirmative,—explaining that it possesses the property of reproducing itself in all the places where it has once occupied, and where it has never been absolutely annihilated,—as often as the climate invites its reproduction. He maintains, nevertheless, that a concurrence of various physical causes and local circumstances is requisite to this reproduction; and that it is quite possible, by resisting the operation of these causes, in counteracting or amending the habits of the natives, to extinguish the disease altogether. He maintains, however, the necessity of quarantine and other means of restriction.

---

* An Historical Account of the Several Plagues, &c. p. 87. London, 1755.

† Ibid. p. 90.

Since the time now referred to, the question of the contagious or non-contagious nature of plague has been agitated with considerable keenness by Dr Maclean, whose work was noticed, and whose views were canvassed at some length in the twenty-third volume of this Journal. The question has been again brought under notice by Dr Bowring and Mr Holroyd, both of whom have examined with great freedom the grounds on which the contagious nature of the disease has been defended.

Dr Bowring has been led, from his observations while residing in the East as a politico-commercial agent, to call in question the correctness of many of the opinions current on the contagious character of the oriental plague, and the measures usually adopted to prevent its propagation. He has observed particularly, that not only is the disease indigenous or endemial in Egypt, (p. 3), but the usual means employed and supposed to be efficacious in arresting its progress are altogether inert and inadequate. He has also observed, that, if the disease be really communicable from one person to another, it is so to a degree infinitely less than is commonly believed.

"Wherever I had occasion," says Dr Bowring, "to witness the introduction or the progress of the disease, its introduction was spontaneous, indigenous, and endemic; its progress never traceable from patient to patient; it broke out in districts remote from one another, between which there had been no communication; and while my own observation surrounded me on the one hand with thousands and tens of thousands of cases in which the most intimate intercourse with persons ill or dead of the plague, the dwelling in their houses, the wearing their garments, the sleeping in their beds, were not followed by disease in any shape, I was called on the other, to listen to stories as evidence of the contagiousness of plague, so puerile, so ridiculous, that nothing but oriental credulity could by possibility be satisfied by them."—Pp. 3 and 4.

The greater part, indeed, if not the whole of the authorities quoted both by Dr Bowring and Mr Holroyd concur in regarding the plague as the indigenous or native offspring of Egypt. Thus, Mr Abbott, one of the physicians or surgeons to the fleet of the Pacha, to the question, whether the plague is indigenous in Egypt, or of foreign importation, answers, that he thinks it indigenous; that he believes Egypt never entirely free from plague; and that he has never known it to be imported. (Dr Bowring, p. 29).

To the same effect is the testimony given by Dr Thomas Lesslie Gregson, surgeon-in-chief to the Naval, Military, and Civil Hospital at Alexandria, as quoted by Mr Holroyd. Dr Gregson believes Egypt to be never free from plague; and he thinks that it originates from miasmatic influence, from finding its ravages cease about the 20th of June, when there is no longer humidity to favour the decomposition of animal or vegetable substances.

Dr Pruner, chief physician to the Central Hospital at Cairo, is the only person whose testimony throws any doubt on this point. While he expresses his belief that Egypt is never entirely free from the plague, he thinks that the disease is propagated both by contagion and by infection. He allows, however, that he never saw the disease propagated by contagion. (Mr Holroyd, p. 25.)

All these authorities, therefore, agree with Prosper Alpinus, Dr Arbuthnot, the anonymous author, and Mr Ingram, in considering plague as endemial or indigenous in Egypt.

Dr Bowring knew numberless instances in which persons most freely exposed to the sick did not contract the distemper; and among the numbers so exposed, a much smaller proportion actually were attacked by the disease, than might have reasonably been expected, had the disease been really and truly contagious. Dr Bowring, indeed, maintains, that everything that takes place in Egypt, shows that it is by no means an easy matter to communicate the plague. Conversely, he remarked that all the boasted precautions which have been so confidently represented to be successful in excluding plague from places in which it had not been known to prevail, were entirely inefficient in preventing the distemper from spreading. Thus, the cordons, the practice of secluding healthy places, and all the cumbrous and expensive machinery of the quarantine establishments, he observed to be often inefficient in preserving places from attacks of plague. Even the precautions employed by individuals to prevent themselves from being attacked, he ofttimes knew to be inadequate; and persons who had been most scrupulous in observing all the prescribed rules to guard against the invasion of the disorder, he had seen attacked and destroyed by it.

Mr Holroyd takes very nearly the same view of the nature of plague. Without positively denying its contagious characters, he has adduced evidence, as we have already seen, to prove that plague is never absent from Egypt,—in other words, that it is endemial in that country; that in a large proportion of cases the disease is not communicated from the sick to the sound in contact or close intercourse with them; that all quarantine restrictions have hitherto been found ineffectual in preventing its transition from one place to another; and that in general the quarantine regulations are not only serious impediments on commerce and travelling, but seem to be productive of more harm than benefit.

It is very true that both authors, but especially Mr Holroyd, have been forcibly impressed with the evils of the quarantine system, and the bad effects of the doctrine of contagion, by having been made to witness and to feel these evils; and this, perhaps, is an argument that might be turned against their own cause as much as in its favour, since it might be said that the restraints of the quarantine and the lazaretto had led them to consider whether it were not practicable to diminish the force and rigour of the doctrine of contagion, by which all the evils are produced. Though we can easily understand the force of this sort of argument, and can perceive that where men are subjected to restraints always severe, and ofttimes arbitrary and capricious in appearance, and to travellers and commercial persons particularly inconvenient, they will be quicksighted in inquiring into the reasons of such restraints, and where they wish them removed or alleviated, will naturally take views as opposite as possible to those who have a desire to continue these restraints, and often an interest in continuing them; yet it cannot be denied that so much evidence and so many facts have been adduced by persons of competent knowledge, and in a reasonable degree unbiassed in their views, that the whole system of the restrictions upon intercourse on account of plague would require to be carefully revised.

There seems, indeed, something singularly inconsistent in the whole of the hygienic and police regulations on the intercourse of nations from the dread of plague. We have in the midst of our population, and in every European country, two diseases, viz. small-pox and typhous fever—not much less fatal and unmanageable than plague;—one of them supposed by many physicians to be merely a milder form of plague—both, so far as can be discovered, very frequently, if not always, propagated by contagion; and yet against these, or persons labouring under them, we have no police regulations, no restraints hardly, and very few precautions even; and the sound are allowed to associate with the sick in the freest manner, with the effect of propagating both diseases to the greatest extent, and in all directions, and to the great increase of the ordinary and necessary mortality, as well as the increased misery and multiplied sufferings of that division of society most exposed to the ravages of these distempers. There is also little doubt, from manifold experience, that precautions and restraints are extremely effectual in preventing the diffusion of both of these diseases. Yet, while we neglect all precaution and restraints, and even in some respects act as if we wished to propagate typhous fever and small-pox, we impose the most rigorous system of quarantine regulations to prevent the propagation and the introduction of plague, over which it is very uncertain whether we really possess this control or not.

The whole of the evidence adduced by both authors as to persons in free, frequent, and close intercourse with the sick not being attacked by the disease, is quite satisfactory and conclusive as to that fact. But, however clearly this point were established, it would contribute but little to prove that plague is not propagated by contagion. As we have already said, there are several diseases unequivocally and universally admitted to be contagious; yet, of the persons exposed to these, by no means the whole are attacked; and the exemption of such persons has been regarded as rather proving that they were at the time unsusceptible of the disorder, than that the disorder is not contagious. There is, of course, a limit to this mode and form of immunity; for, if it were observed that but a small proportion of the persons freely and frequently exposed to, or in intercourse with, the sick, was attacked, it would reasonably be inferred, either that the disease is but very slightly contagious, or that it is not contagious at all. The latter conclusion would be rendered as positive and legitimate as any conclusion in medical reasoning and on pathological subjects, and especially etiological subjects can be rendered, providing it could be proved, or even rendered highly probable, that all, both sick and sound, had been or were habitually exposed to the influence of certain common causes, which might operate alike, or at least, excluding circumstances of length of time, idiosyncrasy, &c. in the same manner upon all so exposed.

This appears to us to be the predicament in which this part of the question of the contagious nature of plague is placed. It may be safely granted, that many persons fully exposed, and in frequent close intercourse with the sick, are not attacked, and escape the distemper. It will still remain to be proved whether this immunity should be ascribed to the absence of contagion, or to the want of susceptibility. This point can be determined in a probable way, in no other mode than by ascertaining among any given number of persons exposed, what is the number and proportion of those who escape.

"I made it my business," says Dr Bowring, "to inquire what had been the proceedings of the medical military authorities when the plague broke out in the army of the Pacha of Egypt. I found that it has been the usage to remove the troops from the locality in which the disease has made its appearance, and that their removal, so far from spreading the disease into the district to which the troops have marched, has in all cases arrested its progress. A memorable instance occurred in 1835, when many soldiers among the troops at Abouzabel were attacked by plague. The regiment was ordered a few miles off into the desert, and the malady wholly ceased."—P. 21.

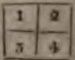
Afterwards the following facts are given:

"The cases in which contact with persons having the plague, or with the corpses of those who have died of the plague, has not communicated the disease, are so abundant as to show that the contagious principle cannot at all events be a very active one, at least on ordinary occasions. Mothers frequently die of the disease without communicating it to their sucking children—husbands and wives have the malady while in a state of habitual cohabitation without conveying it to their partners. Plague patients constantly expire in the arms of others, and no evil consequence results, and it is scarcely possible to have been long a resident in the Levant without being an ocular witness of cases where to be in the presence of, and even to have handled, a plague patient, has led to no injurious consequences. A case occurred at Alexandria, where a Frank lady (Mrs Portalis) who had been keeping a strict quarantine, was attacked by and died of plague (I think in 1835.) It was not known at first under what disease she was suffering, and being a person highly esteemed, and of some consideration in the place, she was visited by the principal ladies of the town, who did her many friendly offices, embraced her, lent her such personal attentions as her indisposition required, without any idea of the character of her malady. She expired the day after she had received these visits, but no one of the visitors was attacked by plague.

"Even those who contend most strenuously for the contagiousness of the plague, have not ventured to say that they have been able to trace it from person to person. Cases of the appearance of the disease after contact or communication with plague patients may no doubt occur; but it by no means follows, that the contact or communication was the immediate cause of the spread of the disease. What is wanting to support the popular theory and to justify the quarantine regulations is, to prove that the plague is not of spontaneous or endemic growth—that it is communicated by a specific contagion only—and that Lazzaret arrangements can prevent its progress. I apprehend that neither of these theories can be supported by facts sufficiently numerous to overthrow the immense multitude of facts which appear to demonstrate, first, that the primary cases of plague are not imported, but indigenous in the spots where it exhibits itself—that its progress is not to be tracked from one diseased person to another, and that the establishment of cordons or Lazzarets has not prevented the introduction or the spread of the disease."—Pp. 22, 23.

On the same subject the following answers to questions 4 and 5 proposed to Mr Abbot, give similar evidence:

"Question 4. Have you known any cases where contact with a plague patient has not communicated the disease?

"Answer 4. Yes, many! I myself not only touched my patients every quarter of an hour, but obliged my assistants not only to touch them but to sit on the beds of the patients, and there to remain until relieved (hourly) to administer the remedies prescribed. Neither I nor my assistants were attacked by the disease. The hospital on board my vessel '*Aboukir*,' was only constructed to hold eight persons, four below and four above, thus  separated only by rails about three inches apart, so that in reality four persons were lying in one bed, and yet not one of the many other patients admitted into this hospital during the time (nearly three months) the plague patients were there, had the slightest symptoms of the disease. I never adopted any kind of quarantine or any fumigations. The '*Aboukir*' was a line of battle ship of eighty-four guns, and had at that time upwards of 1000 men aboard, suffering under the usually considered predisposing causes; such as discontent at being confined on board, and separated from their families, whom they had left at a day's notice, without money or visible means of subsistence; depression from anxiety concerning their families, of whose health they had no means of inquiry, &c. &c. &c.

"Question 5. Have you known cases in which contact, examination, handling, or dissection by medical men, has not been attended with danger to the practitioner?

"Answer 5. Partly answered above (in No. 4.) I have known several medical men, who, while the plague was raging with its greatest violence, persisted in making *post mortem* examinations of plague patients, without taking the disease, and yet when the disease had nearly ceased, (indeed, after all quarantine had ceased, and every one in full pratique) they have been attacked and carried off by it. Could it have happened by contact? is it not more probable it was caught by infection, or the common cause of the disease? Mr Laidlaw, a practitioner of first-rate abilities, attended many cases of plague, during the whole existence of disease in this country. He established a hospital for the reception of the English, and upon one, a captain of a merchant ship, he tied the external iliac artery, the femoral artery having given way, in consequence of the great sloughing of the bubo and surrounding parts. The patient recovered, and Mr Laidlaw did not take the disease; neither gloves of oiled silk, or any other absurdities were used."—Pp. 30, 31.

The facts collected from similar sources by Mr Holroyd furnish evidence much to the same effect.

"Dr Gregson, Mr Abbot, and Dr Pruner assert that they have all had many plague patients under their care, and have handled them. Dr Gregson made autopsies of many oxen, and Dr Pruner of many persons who had died of plague, and yet *all* three escaped an attack. The history of Lardoni, the contagionist, is curious, as he not only used every the most minute precautions against receiving the plague by contagion, but when attacked asserted at first most positively that it was impossible that he could be suffering from the disease; he eventually became convinced of the nature of his malady on the third day, and died. Dr Pruner mentions that two classes of medical men were principally attacked, the old exhausted men, and young men freshly arrived from Europe.

"If cohabitation take place between parties, either of whom is affected with plague, and the disease be really contagious, we can scarcely suppose but that it would be communicated from one to the other. But when I put the question to Dr Gregson, 'Have you known the plague to have been communicated by sexual intercourse?' His answer is, 'No, I have known many cases to the contrary.' Mr Abbot remarks that he has never known the plague so communicated; 'but,' he says, 'a case came to my knowledge of a man having connection with a female ill with the plague, and he escaped the disease.' Signor Luigi Casolani, Captain of the Lazzaret of Malta, mentioned an instance of a gentleman he knew in Malta, whose wife died of the plague; and the husband, who had had connection with her just previous to the appearance of the symptoms, was never attacked.

"The cases of the attendants in the Marine Hospital of Alexandria, mentioned by Dr Gregson and Mr Abbot, are very interesting. These persons resided in an Arab village, where the plague was epidemic. They were subsequently in attendance at the hospital, communicated with the patients most freely and extensively, occupied beds in the same wards, and one (Dr Gregson's dresser) is reported to have touched at least two hundred patients, and indirectly, through the other Infirmarians, to have communicated with the whole hospital; and yet the disease did not spread, but was confined exclusively to the individuals who introduced it into the hospital. Dr Pruner mentions 'that attendants and nurses were attacked during epidemical plague.' But he has given us two instances, 'of a Greek lady and Jewess, who were attended by many persons, none of whom were attacked;' and Dr Gregson remarks upon, and Mr Abbot alludes to the case of Madame Portalis, who, while observing quarantine in Alexandria, was attacked with the plague and died, and who throughout all her illness was attended by her daughters, all of whom escaped the disease."—Pp. 36–37.

The facts now mentioned seem adequate to prove that the disease is not nearly so communicable as it has been commonly believed. It seems singular to deny all at once the doctrine of contagion, as to a disease which has been so generally and so long reputed contagious; but it must be observed, that the facts adduced by the present authors are quite similar to those which we have already shown were adduced by Alpinus, Dr Arbuthnot, the anonymous writer, and Mr Dale Ingram; and, as they cannot therefore be regarded as either new or unheard of, they are accordingly entitled to the greater share of attention. Of their correctness and their force and conclusiveness, however, a judgment must be formed, not merely from looking at them alone, but also by taking into consideration the connected proposition which has been so often brought forward by the same class of authors, on the spontaneous origin of plague, and on its indigenous and endemial character in Egypt. If, indeed, this property of plague can be clearly and satisfactorily proved; if it can be shown that plague is the indigenous product of Egypt, then the necessity of disproving its contagious nature almost, if not altogether, disappears. If the circumstances of the soil, the climate, and the other physical characters of Lower Egypt are really sufficient to engender among its population such a disease as pestilence, then it must follow, that, as all its inhabitants are equally exposed to the operation of these circumstances, what is sufficient to produce the disease in 10 or 100 persons, is equally adequate to produce it in 10,000 or 100,000, or, in short, in any imaginable number. To the formation of a disease arising in such circumstances, it is quite superfluous to have recourse to the idea of contagion, or something emanating from the persons of the sick and transmitted to those of the sound. Transmission of such a disease, or rather of the supposed cause of such a disease, is altogether gratuitous; and, indeed, the doctrine falls to the ground by the want of power to sustain itself.

Even the illogical and cumbrous doctrine of contingent and accessory contagion, we regard as not less unnecessary and gratuitous. This doctrine is maintained by Dr Pruner alone among all the medical anthorities cited in the present tracts.

"Dr Pruner differs slightly, though not materially, from Dr Gregson and Mr Abbot. His evidence is remarkable. He believes that the plague is propagated by contagion and infection;' and then immediately relates two instances of plague, in which the parties were visited, attended, and in contact with very many persons, and yet, in spite of no precautions having been observed, no one was attacked with the disease. As Dr Pruner's answer was not so satisfactory as I could have wished, I put another question to him: 'Have you ever seen the disease propagated by contagion?' His answer is, 'No: I never saw it, but I believe it is capable of being proagated by contagion.' Now Dr Pruner has stated that 'he saw one hundred and forty cases of plague,' and yet in this large number he was not (though impressed with the belief of contagion) able to detect a single case of a patient having received the disease in this manner. Dr Laidlaw says, 'that if the plague is propagated by contagion, (and this I by no means deny in toto,) yet, it has been greatly exaggerated, and that so far from its following as a general rule, that persons exposed to the contact of the infected are always or generally attacked, it ought rather to be considered as the exception.' In all this Dr Pruner seems to agree with Dr Laidlaw. Dr Pruner believes that communication by actual contact 'does not always produce the disease: 'very seldom,' he says, 'if the contact is slight and if there is no epidemical disposition for the plague,' Finally, Dr Pruner has 'very frequently' seen cases of plague 'which could not be traced to communication with infected persons.' In addition to these facts I may mention that Clot Bey, who has seen more of the plague probably than any medical man living, is decidedly opposed to and ridicules the doctrine of contagion. The only physician of any reputation in the East who at present advocates this doctrine is Dr Bulard; but, as the opinions of this gentleman appear to have been formed in a very equivocal manner, I shall merely call your attention to what has been said of him by Dr Bowring, and to an article published on the 10th of November 1838, in the Journal des Debats, leaving you to judge whether he be truly and conscientiously a supporter of the notion, that plague is capable of being propagated by contagion."—Pp. 32, 33.

If all the views stated by these two authors be well-founded, it cannot be wonderful that they should regard all quarantine regulations and all the expensive and cumbrous machinery of the quarantine and lazaretto restraints as not only unnecessary but highly injurious. It cannot also be matter of wonder, if these views be well-founded, and if the facts on which they rest be correctly ascertained and not open to fallacy, to see these gentlemen infer that quarantine restraints are altogether useless in preventing either the introduction or the propagation of the disease. From the united testimony of all the authorities mentioned, it appears that these restraints are altogether ineffectual; and it is stated by several of the witnesses, that plague has appeared and spread in defiance of the strictest system of prohibited intercourse. This gives a very different view of the effect of seclusion and separation from that which has been hitherto maintained; for we have been made to believe in the complete efficacy of separation and seclusion as a prophylactic against the inroads of pestilence. On this point, however, it is proper to hear what Dr Bowring says.

"Have Lazzarets, then, the best, the most rigid, succeeded in shutting out or keeping down the plague? By no means. Odessa has frequently been quoted as having one of the best organized quarantine establishments in the world—certainly one of the severest. Yet, not long ago, the plague broke out in the Lazzaret—entered the town—destroyed a number of inhabitants, and ceased at a particular season as it usually does. Quarantines have been introduced during the last seven years, by Mehemet Ali, into his dominions in Syria and Egypt. They have been placed under the control of the European Consuls, and ample funds have been given by the Viceroy, both for the erection of proper edifices and for the payment of the necessary salaries. Has the plague, in consequence of these arrangements, visited Alexandria less than it did before? By no means. Have the quarantines protected Damietta—or Rosetta—or Jaffa, on the coasts—Damascus or Jerusalem, or Cairo in the interior? Nobody can pretend they have. And if looked into, I apprehend it will be found not only that they have been no barrier to the progress of the disease, but that they have created, concentrated, and by concentration, diffused the malady. Many of the Lazzarets are in spots singularly unhealthy; at Beyrout I found, not only that many persons who had arrived in good health, had perished in the Lazzarets, of the plague; but that many had died of dysentery and other disorders, from which they were perfectly free when they entered. No plan could be devised more likely to create perilous or contagious elements, than bringing suffering and diseased people together—creating about them a deleterious atmosphere, and delivering them over to the annoyance of an oriental quarantine. In the Lazzaret at Syria, for example, where the exactions are monstrous, and where lately there was not even a waterproof roof to shelter an invalid, I have seen a person come out of his imprisonment having had his garments devoured by rats, and his person disfigured by multitudinous vermin."—Pp. 12–13.

Supposing these statements to be well-founded, and we are entitled to believe that Dr Bowring is neither inventing, distorting, or exaggerating facts, it results that quarantines, instead of being beneficial, are the sources of much aggravated misery and distress. We are aware that many of their defenders will say at once these are not the necessary evils of quarantine and lazaretto establishments; these are all accessary and accidental evils; abuses; and might with proper care be obviated. To a certain extent this may be admitted; and it will be granted that, where large spacious apartments are provided with sufficient means for air and exercise, several of the calamities here detailed might be obviated. But it must be remarked, that the privation of personal liberty is at all times a very serious evil, and in no circumstances more so than when individuals, to all appearance in good health, are involuntarily placed and retained in circumstances where health may be impaired, the mind deprived of its cheerfulness and energy, and where possibly all the causes of sickliness may operate with tenfold force. For any thing known to the contrary, the disease may arise in consequence of distempered states of the atmosphere, or from miasmatic or miasmato-climatic causes operating conjointly; and if such be the fact, why, it is clear to demonstration, that the confinement, the inaction, the imperfect ventilation, and the other discomforts and inconveniences necessarily attendant on confinement in a lazaretto, are most likely to induce the distemper where it had not any chance of appearing.

Further, when lazarettoes established by public authority are so inadequate to prevent the introduction and propagation of plague, and are likely to be productive of so much evil and calamity, Dr Bowring argues that there is still less chance that private establishments will be adequate to the purpose, or will be more free from other sources of inconvenience and danger.

"If the cordons, quarantines, and lazzarets, which have been established by State authority, have failed to give any protection against the inroads of the plague, still less have the private, or local quarantines afforded that protection. It has indeed been often averred that the keeping a strict quarantine has been found in all cases to be a security against the disease. I doubt if ever so bold, so mendacious an averment has obtained so universal a currency and so heedless an admission. It has passed from lip to lip, and book to book as an undisputed fact, that Europeans in the Levant run no risk from plague, in consequence of their shutting themselves up in their houses during the epoch of the disease. And yet hundreds of examples would crowd upon the diligent inquirer, showing that no quarantine, however rigid, could at all times be depended on for protection. It is true, that the risk run by Europeans in times when the plague is prevalent, is small compared to that of the natives; but this applies not to plague alone, but to every other disease, whether contagious, infectious, epidemic, endemic, or sporadic. In cholera or influenza, in dysentery and ophthalmia, the proportion of sufferers among the indigenous population is far greater than among Europeans in general. The better food, clothing, and lodging—the greater prudence—the more prompt intervention of medical skill, give to more opulent classes, among which a majority of the Frank population are found, far more securities against diseases, than the less fortunate classes of society can command. The question is not whether the Europeans suffer less than the native population from plague, but whether the quarantine precautions taken by Europeans are the causes of their greater safety.

"I am disposed to attach little value to individual or domestic isolation. During the raging of a pestilential disease, prudence, temperance, and quiet, are, no doubt, valuable auxiliaries for self-preservation, by diminishing the dangers of the epidemy, be they what they may. It would be a foolish daring not to avoid rather than seek the districts where the disease is exercising its ravages; as there, no doubt, a deteriorated atmosphere must be the consequence of its presence; and that atmosphere may in itself be an element of danger. But quarantines alone, especially where there has been no prudential change in the habits of life, would appear to have no favourable effect in excluding the malady. During the plague of 1835, the Harem of the Pacha of Egypt, consisted of about 300 persons; notwithstanding the severest cordon, the plague entered, and seven persons died within the cordon. The cordon itself was composed of 500 persons; these were in constant contact with the town where the plague was violently raging; and of these 500 only three died, so that the proportion of those who perished within the cordon, to those who perished without, was as 4 to 1. I only adduce this fact as an isolated instance, not contending by any means that the general proportion of those within cordons who are attacked by plague is greater than those without; but when the cases are carefully collected, which are presumed to show the efficiency of cordons, but which in fact prove nothing, unless the safety of the parties can be traced *to* the protective power of the cordon, it is very important, that the cases having a different character should be fairly stated. The plague has about it much that is unaccountable, and seemingly capricious. The theory of specific contagion may possibly explain some of its influences, but it leaves the greater number unaccounted for. No theory perhaps explains so few of the phenomena of plague; and on that ground alone a more thorough investigation is demanded by science, by philanthropy, and by even enlarged self-interest."—Pp. 16–18.

From the information communicated by Mr Holroyd, it appears that, according to the observation of that gentleman and his witnesses, quarantine establishments and restrictions are not only inefficient in preventing the introduction and propagation of plague, but they are positively injurious, by contributing to the propagation of the disease.

"With regard to the inutility of quarantines, especially where the plague actually exists, Dr Gregson, Mr Abbot, and Dr Pruner coincide in every respect. They all observe that, notwithstanding Europeans are accustomed to maintain what they consider to be the proper and only precautions for preventing the ingress of the plague, instances from time to time occur of their being attacked and falling victims to the malady.

"Dr Gregson speaks very forcibly upon this subject. He says, 'I consider them (quarantines) inefficient, and that from their oppressive and partial operation they have, instead of diminishing, propagated the disease; and hundreds have been sacrificed by being torn from their homes, and thrust into crowded, overcharged, and tainted lazzarets.' Mr Abbot is opposed to quarantines, because he thinks the disease 'is infectious and not contagious.' The opinion of Dr Pruner is curious, as he first considers quarantine to be 'no safeguard at all against the plague;' but immediately goes on to say 'it can only be a safeguard in countries where the disease is not endemical.' Dr Pruner has, however, given us an instance in his answer to the preceding question of 'an Italian who kept strict quarantine, but who was nevertheless attacked with the disease and died.' Clot Bey has expressed himself very strongly on the subject of quarantines, and entirely corroborates the views of these gentlemen. In speaking of Crete, he says, 'Can it be affirmed, then, that, if for six years Crete has been exempt from the scourge, she owes her healthy condition to the establishment of quarantines? This cannot be reasonably pretended, but we shall see. My wishes are truly sincere that the measures adopted may for ever be the safeguard of illustrious Crete; but unfortunately I fear that their fulfilment is impossible, for my opinion is that the plague, being endemic throughout the Levant, spreads itself by the influence of atmospheric causes. I do not advocate the abolition of quarantines; they are to a certain extent useful; but I should like to see them freed from those thousand and one fiscal practices, which are antiquated and vexatious; I should like to see the interests most important to science, commerce, and humanity unshackled by self-interest.' Clot Bey then remarks on the quarantine measures adopted in Crete, but deplores the folly of making vessels arriving from Greece (herself under sanatory ragulations) undergo quarantine upon their arrival in that island, and *vice versa*."—Pp. 34–35.

If these statements be well-founded, what a prodigious amount of mischief and mortality has been produced by quarantine establishments during the last century. In what a pernicious delusion have the governments of most European countries been hitherto involved. It seems as if they had laboured with the most persevering diligence and the most perverted ingenuity to aggravate and multiply a thousand-fold all the natural horrors and calamities of pestilence. Sufficiently severe and distressing are the whole of these calamities by themselves; and it is scarcely possible to imagine a more dreadful scene than that of plague raging in the midst of a populous city; the distemper striking down its victims in family after family, house after house, and street after street, until the sound are too few to aid the sick, and the living not numerous enough to bury the dead; until whole houses and streets are a wilderness, and all who are able to flee desert the devoted place. Yet gloomy as are these natural evils, they are trifling compared to those created by the ingenious cruelty of mankind;—the red-cross on the doors and windows of infected dwellings; the forcible shutting up of houses; the lines of armed sentinels; and all the usual array of means to prevent intercourse between those already attacked and those who are on the eve of being attacked.

> "Mute the voice of joy,
> And hushed the clamour of the busy world.
> Empty the streets with uncouth verdure clad,
> Into the worst of deserts sudden turned
> The cheerful haunt of men; unless escaped
> From the doomed house where matchless horror reigns.

Shut up by barbarous fear the smitten wretch,
With frenzy wild breaks loose; and loud to Heaven
Screaming, the dreadful policy arraigns,
Inhuman and unwise. The sullen door
Yet uninfected, on its cautious hinge,
Fearing to turn, abhors society.
Dependents, friends, relations, love himself,
Savaged by woe, forget the tender tie,
The sweet engagement of the feeling heart.
But vain their selfish care: the circling sky,
The wide enlivening air, is full of fate;
And struck by turns, in solitary pangs,
They fall unblest, untended, and unmourned.
Thus, o'er the prostrate city, black Despair
Extends her raven wing; while to complete
The scene of desolation, stretched around,
The grim guards stand denying all retreat,
And give the flying wretch a better death."

To us it appears that it would be more eligible to dispense with all quarantine regulations, than to incur the risk of aggravating to so great an amount all the natural evils of epidemic disease. Has the mortality, we would ask, in any given case, been greater where no means of precaution or prophylaxis were used, and where they were adopted and enforced in the most rigorous manner? From the history of various plague epidemics, for instance, those of London, Marseilles in 1721, Nimeguen, and those of the cities of the Levant, it appears that it is of little moment whether prophylactic means be used or not; and it seems most natural to think that, wherever the disease has arisen spontaneously, the safe plan is not to confine and shut up the inhabitants in their houses, but to allow them to disperse themselves as widely as possible over the open country. Clarendon remarked, that, after the cessation of the last great plague in London, he missed very few of his friends and associates who had lived in the best ventilated parts of the city; and the mortality had fallen chiefly on the more indigent classes, who were living huddled together in dense masses in the crowded parts of the town.

Another reason for abolishing altogether quarantine regulations, such as they exist at present, is the circumstance of their excessive severity, and the rigour with which they are attempted to be enforced. Wherever laws are severe, many attempts are made to evade them, and it is difficult to enforce them; and often they are carried into effect very partially. This we have long suspected to be the case with the quarantine laws; and among instances of this, one is given by Mr Holroyd, and another is given by Dr Lesslie Gregson.

"Soon after the appearance of the plague at Beyrout, Hekekyan Effendi (now Hekekyan Bey) accompanied by Mucktar Bey, then Minister of Public Instruction, arrived there from Alexandria in a vessel of war belonging to Mahomed Ali Pacha. The plague at this time was, as I have already said, raging at the latter place. They left the ship and proceeded to the shore, intending to land; but Mahmood Bey, the Governor of Beyrout, having received intelligence of their intention, came to the shore and requested them to remain in their habit. He then retired, but shortly returned with a guard of soldiers to accompany them, and see they did not communicate with the people of the town; and this guard conducted Hekekyan and Mucktar Beys through the town to Mahmood Bey's house, a distance of about a quarter of a mile. The two Beys entered the Governor's divan, and sat on chairs; coffee was brought, placed upon the floor, and each took a cup. The three Beys were in consultation for some time respecting the best means of preventing the spread of the plague, and eventually they made arrangements for placing the Cordon around Beyroot. Afterwards Hekekyan and Mucktar Beys returned to the shore, accompanied by the escort, and embarked for their vessel. Here they remained eight days, at the end of which time, the mules having arrived, they landed outside the Cordon and went to Deir el Kammar, the residence of the Emeer Besheer, thence to Cornail, the Pacha's coal mines, and, after being in Mount Lebanon five days, returned to Beyrout, embarked outside the Cordon, and proceeded to their vessel. They sailed without loss of time for Alexandria, arrived there seven days afterwards, and were admitted to pratique immediately.

" The two Beys communicated with Alexandria, where the plague was raging, previous to their sailing for Beyrout, and it appears they were not placed in quarantine upon their arrival in the latter port. They most completely communicated with Beyrout, where the plague was reported to be, and yet were not placed in quarantine upon their return to Alexandria. And lastly, they most completely communicated with Deir el Kammar and other parts of Mount Lebanon, the inhabitants of which places had daily intercourse with Beyrout after the existence of plague was discovered, and previously to the establishment of the Cordon. Fear, therefore, might reasonably have been entertained of the two Beys having received the infection of plague, and of their giving it to others. Besides, after communicating with Beyrout, they remained on board eight days, and then landed and spent five days in Mount Lebanon; and as the quarantine at the Pines was of only seven days' duration, persons might have passed the prescribed period of detention and communicated with Deir el Kammar and other places about the same time that these emissaries of the Pacha reached these parts. As the quarantine at Alexandria from Beyrout was twenty-one days for ships of war, ought not the Beys to have been incarcerated upon their return."—Pp. 10–12.

" Do you consider the Board of Health of Alexandria efficient?

" A. No, I consider it inefficient, expensive even to prodigality, tyrannical in its operations. It has even solicited the dismission of zealous medical men, because they had reported the tricks of their emissaries. I may mention the case of Dr Bowring and Clot Bey sailing for Syria, its keeping back the announcement of a case of plague till they sailed, although the case was known three days before; they were not out of sight when the case was declared. I sailed for Syria a few hours afterwards and had eighteen days quarantine. I know the Commission hastened the departure of Clot Bey, he himself having told me that he meant to stop in Alexandria other four days."—P. 19.

It is perfectly clear, that when quarantine regulations are enforced in the manner here represented, they cannot fail to be worse than useless, and they can be of no service in preventing the introduction of a disease alleged to be contagious; and the sooner the whole system by which they are enforced is abolished, it will be the better for the countries in which they are carried on.

But it may be argued, if the large, the complicated, and the expensive machinery of public quarantines be useless and injurious, surely there must be less objection to individual prophylactic measures. To this it may be answered, that certainly every one in a free community is fully entitled to adopt all necessary precautions, and all prophylactic measures, the execution of which does not interfere with the safety or the comfort of others. It is also manifest, that one of the great evils of public quarantine is, that they prevent individuals in several instances from having recourse to those means which each may deem requisite for his security and protection amidst the visitation of epidemic sickliness. In short, if precautions or seclusion be adopted, we think it would be much more judicious to leave them to the choice of individuals, than to adopt a great general system of compulsory seclusion and segregation. But even the efficacy of these precautions may very justly be questioned; and the experience of every plague epidemic furnishes numerous instances in which all the precautions employed were unavailing. Without going back to such instances as are given by De Foe and Hodges, take, if you please, the following example from Dr Bowring of the precautions of a contagionist, and their efficacy.

"Those who took the greatest precautions were among the sufferers. M. Lardoni was a remarkable instance. He was the most timid of men—he never visited his patients but on horseback, and his appearance is thus described:—'His harness was wholly of unsusceptible materials, his saddle closely covered with oilcloth, his stirrups were braided, and his reins made with filaments of the date tree; he had a huge oilskin cloak in the shape of a sack, which rose above his head and descended beneath his feet; he was always escorted by four servants, one before, one behind, and one at each side, so that no person could approach him.' A thousand other ridiculous precautions were adopted by him; they were all in vain; he was attacked, though, for two days after the attack, he declared it was impossible it should be the plague; on the third he announced that it was really the dreaded calamity, and died soon after."—P. 27.

In short, the actual evils of the present quarantine system are greater than those of the plague, which they are meant to prevent. Many of their rules and restrictions, and many of their ceremonies, especially as to letters and various articles of merchandize, are manifestly useless, even on the supposition that the plague is really contagious. Many of them are also insufficient, and quite incapable of preventing the disease from appearing. Thirdly, several of their rules are in the highest degree pernicious, and appear rather calculated to propagate the disease than to prevent its diffusion. Fourthly, several of the observances and practices enjoined by the quarantine regulations are inconsistent with others, unreasonable in themselves, and by no means uniform in principle. And lastly, the impediments which they cause to commerce and to travelling are so great and so insufferable, that nothing but a clearly and well established necessity can justify their continuance.

We have been obliged to take rather a hasty, and perhaps an imperfect view of this subject at present, chiefly suggested by the tracts of Dr Bowring and Mr Holroyd. We shall have occasion, however, again to revert to the subject in considering the publications of Clot Bey and M. Bulard; and we shall then, in all probability, give some of the points a more full and thorough consideration. Meanwhile, we must say, that so many arguments have been adduced tending to invalidate the doctrine of contagion as to plague, that it is impossible to look on that disease in the same light in which it has hitherto been viewed.

ART. V.—1. *Practical Observations on Diseases of Women, illustrated with Cases and Explanatory Plates.* By WILLIAM JONES, M. R. C. S., Surgeon to the Free Dispensary and Infirmary for Women, &c. &c. London, 1839. Pp. 226.

2. *A Practical Treatise on the Function and Diseases of the Unimpregnated Womb, illustrated by Plates, &c.; with a Chapter on Leucorrhœa, Fluor albus, or Weakness.* By CHARLES WALLER, M.D., Consulting Physician-Accoucheur to the London Midwifery Institution, &c. &c. London, 1840. Pp. 200.

"To treat successfully any disease it is necessary to know it," has long been a maxim in medicine; and the improvements in the means of diagnosis which an enlightened pathology has suggested, have done much to render this a more easy task than once it was. To no class of diseases have pathological researches rendered more valuable service than to those affecting the uterine system; and since men of enlightened understandings and cultivated minds have devoted their talents and their zeal to this branch of the profession, we are in possession of a body of evidence which renders the ascertaining the nature of these diseases more sure, and their treatment more successful.

Amongst the latest aids to our means of examining the state of the uterus in disease is the speculum, an instrument which serves to bring within the reach of vision, a part of the body, on the condition of which the ordinary means of examination are unable to furnish information. Like every other discovery, however, its value may be exaggerated; for whilst some rationally confine its employment to those cases only where the ordinary means of examination prove insufficient to ascertain the nature of the disease, others bestow on it the highest encomiums, as a means of examination which ought in every case to be employed. Amongst the latter class is Mr Jones, who, in his anxiety for the universal adoption of this instrument into practice, has allowed his zeal to outstrip his prudence, and, by wishing to prove too much, has greatly weakened his cause. He attributes the loss of many an unfortunate patient, "in the full pride of youth and bloom of womanhood," to the unfortunate prejudices which still exist against the employment of this instrument; and his work is apparently written for the purpose of proving, that no means are sufficient to detect disease of the uterus, unless conjoined with that of the speculum. The greater portion of his volume is therefore devoted to the narration of cases illustrative of the uses of this instrument, but, unfortunately for his theory, the far greater portion of these cases prove that the speculum was employed unnecessarily, and even after its employment, gave no more information than what the symptoms and more usual mode of examination had already detected. In a few of the cases, however, its use was obviously indicated, and its great advantages rendered apparent. The collection of cases is, nevertheless, of considerable value, and, being arranged systematically, easily consulted. These cases also serve to prove, what is but too frequently loosely stated, even in many systematic works, that the same disease may be produced by very various affections of the generative organs. As illustrative of this, we refer more particularly to his series of cases showing the different disorders which may give rise to menorrhagia and leucorrhœa. Mr Jones, however, deserves the thanks of the profession for his zeal in drawing their attention to a means of exploration rather too much neglected in this country.

It may be remarked, that the speculum table, which Mr Jones says he has "contrived for the purpose," and of which he has given a representation, is very similar, but not so convenient as the ordinary speculum table used at the Venereal Hospital at

Paris, and many similar institutions on the Continent. In Mr Jones's table, the legs of the patient are supported at the bend of the knee on a kind of hook or stilts, whilst, in the others, the feet are supported on a kind of shelf in front of the table, which renders the position much less irksome to the patient, and is one far more easily assumed.

Dr Waller, the well known editor of the late edition of Denman's Introduction to the Practice of Midwifery, states in his preface, that the chief subject matter of his work was published in the form of lectures in the Lancet for 1839–40; but that he was induced to collect the scattered materials, in order to present to the profession a condensed account of the more common diseases to which the uterine system is obnoxious.

The subjects treated of in this volume are divided into four heads. 1. The function of the unimpregnated uterus. 2. The diseases of that function. 3. The period of female life designated the turn of life. 4. The diseases of the uterus itself.

The present volume contains a short and succinct practical account of the principal morbid states either of the functions or the structure of the womb, the best methods of distinguishing them, and the means which experience has shown to be the most effectual in removing them, and where this is impracticable of alleviating their effects.

After some observations on the function of the unimpregnated womb, or, in other words, the process of menstruation, Dr Waller proceeds to consider the disorders to which this function is liable, under the separate heads of amenorrhœa, *Emansio mensium*, and its causes, Chlorosis, suppression, acute and chronic, menorrhagia, active and passive, dysmenorrhœa, cessation, and lastly, sterility. He then considers the diseases of the unimpregnated womb, arranging under this head, inflammation, (*Metritis*); and of the orifice and neck of the organ, neuralgia, *Tympanites Uteri*, calculus, worms, moles, hydatids, malignant ulcer, cauliflower excrescence, cancer, fungoid tumour, polypus, and tuberculated uterus; and the volume is concluded with a short sketch of the symptoms and treatment of leucorrhœa.

Though on none of these subjects are the accounts lengthened, the author always gives all the requisite information, and the reader will find that he obtains, in a small compass, a distinct view of the nature and treatment of each disorder.

In treating of menorrhagia, which Dr Waller distinguishes, according to the usual practice of nosologists, into active and passive, he very properly remarks, that " effusions of blood from the womb are not always the result of simple functional disorder; they are the never-failing attendants of many organic diseases. Hemorrhage occurs in polypus, hydatids, cauliflower excrescences, carcinoma, malignant ulcer, *fungus hæmatodes*, &c. Some females, again, lose considerable quantities of blood during pregnancy. The author attends a lady who for the first four months suffers greatly from repeated floodings. It will be apparent, therefore, that in all cases of protracted bleedings from the womb, an examination *per vaginam* must be made, that their nature and cause may be satisfactorily ascertained, as incalculable mischief might be the result of an erroneous opinion in this respect."—Pp. 61, 62.

In delivering the method of treatment, Dr Waller judiciously adheres to the distinction of the disease into active and passive. The former he recommends to be treated altogether as an active hemorrhage, by the use of general blood-letting, perfect rest, low diet, laxative medicines, and the use of refrigerants. This we are glad to see so carefully observed and enforced; for whatever be said to the contrary, we know that many practitioners abstain most religiously from the use of the lancet, and confine the whole treatment to the exhibition of acetate of lead. There is nothing, however, so effectual in checking this form of menorrhagia as general blood-letting; and it is sometimes requisite to carry it to a considerable extent.

Another remedy which Dr Waller recommends, and in recommending which we entirely concur with him, is the use of nitre,—an old remedy, but which is now, perhaps, too much neglected.

In the passive form of the disease a different mode of treatment is indicated; and astringents, sedatives, and tonics, are the chief remedies.

In enumerating the various remedies which have been recommended for the cure of menorrhagia, he mentions a curious circumstance as to the effects of vinegar in suspending for a time the menstrual discharge. A lady informed him that, when she wished to spend an evening from home during the catamenial period, she was in the habit of swallowing a gill of vinegar, which had the invariable effect of stopping the discharge till the following morning, when it regularly and constantly returned. This is a practice which is but too common amongst unmarried females; and several cases have come under our notice where a bad state of health was attributed to the too frequent repetition of this pernicious practice.

Dr Waller appears to place little confidence in the powers of the ergot of rye in menorrhagia; a circumstance to be ascribed, perhaps, in some degree to the form of the preparation, and the mode of administering the agent. He directs half-a-drachm of Battley's essence of ergot to be given only every six or eight hours. To produce a decided impression on the system, it would be necessary to administer the same dose every hour. But the fresh powder diffused through water appears to be a more sure and powerful preparation. He very properly recommends that it should not be trusted to alone.

In administering the superacetate of lead, Dr Waller gives one or two grains with a quarter or half grain of opium every two hours till four doses are taken, then waits several hours, in case unpleasant symptoms manifest themselves, before repeating the medicine in a similar cautious manner.

The unpleasant symptoms consist of griping and colicky pains; and though these are usually obviated by the addition of the opium, it sometimes happens that they are developed with much severity, and cease only on giving up the use of the lead. In such circumstances, Dr Waller thinks it better to wait a little, and, should abdominal pains be complained of, the remedy must be laid aside. If no such unpleasant symptoms ensue after the lapse of three or four hours, the same number of pills may be again prescribed, the patient being, at the same time, closely watched, lest circumstances should occur requiring an immediate discontinuance of the lead. Under these restrictions, with the exercise of proper care, the superacetate he allows to be an invaluable medicine; but he regards it as a dangerous remedy, if used without due caution.

Dysmenorrhœa is one of these diseases, which, more than any other, has been generally treated empirically. This appears to have arisen from the affection having been regarded as a disease, while it should have been considered a symptom only.

Of dysmenorrhœa, Dr Waller distinguishes two species, the first, dysmenorrhœa, with the formation of membranous productions or shreds; the second, dysmenorrhœa without the formation of membranous shreds.

It is well-known that Dr Denman, who first, perhaps, described accurately the first variety, maintained the opinion "that no woman in the habit of forming this membrane has been known to conceive while this habit exists;" and it is also known that this opinion has been very generally repeated by most authors since the time of Dr Denman. This, however, Dr Waller sees occasion to controvert, on the ground of many well authenticated cases which prove the contrary, and two or three under his own care, in which, though membranous menstruation had existed for some time, yet impregnation followed almost immediately after marriage.

The truth is, that sometimes in mild cases marriage and its consequences effect a sort of temporary cure; but the individual is often in danger of various accidents and diseases during the period of pregnancy; and sometimes sudden miscarriage ensues.

What is the cause of this membranous production? Dr Waller informs his readers, that, in some cases, its formation may be attributed to an action of the vessels amounting to inflammation, and here the antiphlogistic treatment, to a certain extent, he allows, will be necessary. Our experience leads us to think, that, in most cases of this variety of dysmenorrhœa, there is no doubt that there is inflammation of the inner or mucous surface of the womb, which, being in several respects similar to a serous membrane, actually assumes the inflammatory state peculiar to these membranes. The orifice of the uterus is then felt tumid, prominent, hard, and painful on pressure; and we know, from examining uteri of this kind after death, that they are highly vascular. In all such cases, depletion by cupping from the loins, as recommended by the author, is the appropriate remedy; and sometimes it is requisite to practise this depletion periodically; for the inflammatory state is chronic. In some instances, we have seen a blood-letting of from eight to twelve ounces, immediately before the approach of the menstrual period, followed by an opiate, productive of great relief; but usually remedies require to be continued for some time, in order to subdue the inflammatory action, and put a stop to the morbid secretion.

One variety of this disorder Dr Waller ascribes to rheumatism of the womb.

On this subject, the work of Mr Jones contains some valuable information, where he points out the connection of this disease with engorgement, with induration, with chronic inflammation, and with scirrhous enlargement of the os, neck, or body of the uterus. The cause being detected, the appropriate treatment soon effects a cure.

The subject of sterility is rather a strange topic in a work professing to treat merely of the diseases of the unimpregnated womb.

It is remarkable that most authors on midwifery attribute sterility to the faults in function or organization of the female organs, and but seldom, or at least in a much smaller degree, to those of the male. A circumstance which Dr Waller relates, however, and the case is by no means uncommon, of a woman not having a family to her first husband, but having one to her second, apparently shows that the male is often as much at fault as the female, and not merely that the uterine system was insensible to the semen of one man, and stimulated by that of another.

The diseases of the unimpregnated womb have apparently received a considerable share of attention from Dr Waller. Inflammation of the uterus, as also that of its os and cervix, are well described, and the treatment recommended founded on sound pathology. Neuralgia of the uterus he treats as a general affection, for which he prescribes tonics, particularly iron, in the form of sulphate, nutritious food, stimulating drinks, strict confinement to the horizontal position, occasional anodynes, especially a full opiate at bed-time, and, if there should be any inflammatory symptoms, a few leeches to the vulva, or cupping to the loins. For the sake of regulating the bowels he prefers the daily administration of an enema, to the practice of giving medicines by the mouth.

On this subject and the use of local depletion, in relieving and removing the pain, we would refer to the interesting case recorded by Dr Scott in the forty-second volume of this Journal (p. 306).

Dr Waller considers the disease described under the name of hydrometra or *ascites uterinus* to be merely a large single hydatid filling and dilating the uterus, as, both in the case which he himself witnessed, and those which are recorded, where the symptoms were particularly described, it was observed that, after the evacuation of the fluid, a membranous bag was expelled.

Among the symptoms attending the corroding or malignant ulcer of the womb, attention is directed to the particular kind of pain which occurs in this variety of uterine disease. This is stated to be of a burning or scalding nature, and to be so pathognomonic of the disease, that, even before an examination is instituted, the disease may be named. In cases, however, in which the presence of this disease is suspected, we agree with Mr Jones in recommending careful examination by the speculum, as the only sure mode of deciding the nature of the disease, and distinguishing it from others with which it may be confounded. Dr Waller believes that the original cause of this malignant ulceration is inflammation, which is not essentially specific in its character, but observes the same laws, and yields to the same treatment as in any ordinary case; and that the specific character which the disease afterwards assumes is determined by the tendency to malignant action which exists in the individual constitution. As the disease makes its appearance most generally about that period of life when the catamenia cease to flow, no uneasiness experienced in the genital organs, at that time of life, should be lightly passed over. As prophylactic measures, Dr Waller advises the general, but especially the local abstraction of blood, and careful attention to the state of the bowels, together with the warm hip-bath; anodynes at bed-time; the frequent use of hot injection into the vagina of decoction of poppy-heads, or of hemlock leaves. When ulceration has taken place, he regards the case as hopeless, though, by the employment of judicious means, some amount of relief may be obtained. The removal, by operation, of the diseased portions, as recommended by Lisfranc, he thinks, might prove successful; but he has never witnessed a case in point.

The cauliflower degeneration, differing from the other malignant diseases, in not being attended with much pain, though apparently a necessarily fatal disease, he regards as, to a certain extent, under the influence of remedies, so that life may be prolonged for a number of years, and rendered comparatively comfortable. The means recommended by the author for this purpose are, strict maintenance of the recumbent posture, venesection, or topical blood-lettings occasionally repeated, cold local applications, the use of laxatives, and spare diet.

Scirrhus, one of the most common and fatal diseases of the uterus, is fully described. Dr Waller has given a plate of a preparation, which shows that this disease occasionally commences in the body of that organ, though this is not its common course. Dr Waller would hesitate to recommend extirpation of the uterus, the success of the cases which have been operated on not being such as to recommend its adoption ; besides, there is not the slightest chance of success, if the organ be attached to the surrounding parts, or if the vagina be involved in the disease.

Nothing of interest is noticed regarding *fungus hæmatodes* or *polypus uteri*. The tuberculated uterus, and the symptoms to which it gives rise, are pretty fully described ; and, though Dr Waller has never seen an instance in which medicine effected a removal of the fleshy tumours, he states that he has been successful in arresting its progress for many years at least. The means which he adopted for this purpose were, general and local bloodlettings, aperients to obviate constipation, mercury in small doses, for the purpose of inducing ptyalism, and confinement to the recumbent posture, so long as uneasiness was experienced. If much restlessness be experienced, an anodyne is recommended at bedtime.

His last chapter is on leucorrhœa, and he justly remarks, that much of the difficulty experienced in the removal of the complaint arises from the want of discriminating the various causes which produce it. These he attributes to five states, 1. Inflammation of the vagina ; 2. Relaxation and debility ; 3. Habit ; 4. Sympathetic irritation ; 5. Uterine leucorrhœa.

On this subject much valuable information is contained in the work of Mr Jones, in his series of cases on leucorrhœa ; and for the illustration of this and various other morbid states of the uterus, we recommend its careful perusal.

Art. VI.—*A Treatise on the Sympathetic Relation between the Stomach and the Brain, and throughout between the Digestive and the Nervous Systems in the Causation and Cure of Diseases ; with an Appendix, containing a few observations on certain points connected with the Treatment of Chronic Disease and its attendant Debility.* By Charles Wightman, M. D., Licentiate of the R. Coll. of Physicians of London, and Resident Physician in Newcastle upon Tyne. London, 1840. 12mo. Pp. 192.

When an English physician publishes a work on any subject in physiology, pathology, or therapeutics, he generally pursues a plan different from that observed by a foreign physician. The latter almost invariably knows what has previously been written on the subject by different authors, or he labours to make himself acquainted with the researches of his predecessors in the same line of inquiry; and after giving a tolerably full and accurate view of what has been already done before his own time, he then submits to the reader the observations, experiments, and researches which he has made personally,—shows wherein he has had occasion to rectify, to extend, or to modify,—and concludes, by giving the general results of the whole amount of information collected by the different individuals, by whom the subject has been investigated.

The English physician, though he sometimes follows this course, by no means does so at all times. Regarding what others have seen, said, thought, or written, he is little solicitous. If he possesses this knowledge, it is well, and perhaps he notices in a cursory manner the share which a few of the leading inquirers have had in elucidating the subject. But if he possesses it not, it is equally well, and he gives himself little concern about the matter. He has made upon a particular subject some observations and reflections, which he thinks are well-founded; he has drawn some conclusions, and made some practical applications, which he believes deserve to be known; and, little solicitous whether any other person has considered the subject, or what has been said by others, he forthwith publishes his own views.

Each of these methods have advantages which do not belong to the other; and though we do not disguise the fact, that we like much to peruse the elaborate and learned treatises of foreign physicians, in which the whole literary history of the subject is clearly and methodically unfolded before our eyes, still we think the original observations of the English physicians often possess a degree and kind of interest, which leaves, perhaps, a deeper, and often a more lasting impression. The foreign monographs are generally more learned and more instructive; the English essays are not unfrequently remarkable for originality, and the truly practical tone of their remarks. The foreign monographs please, by the fulness and comprehensiveness of their facts and arguments, and, by making the reader feel that he sees all that is known on the subject, satisfy curiosity, and often stop all further inquiry. The English essays have the merit of seldom allowing readers of ordinary information to perceive that anything is wanting; and while they give information, which is so easily understood that the reader is astonished that it did not all occur before, they often, especially on a second perusal, suggest subjects for further research, and more detailed inquiry.

The author of the present essay is somewhat in the predicament now mentioned. He is evidently a person of much observation and reflection, and one who has not allowed the cases which he has had occasion to treat, to pass before his eyes without observing them carefully, and thinking on them repeatedly. His attention had been originally directed to the subject by having chosen it for his inaugural dissertation, which was published in 1808, with the title of De Consensu Ventriculum inter et Cerebrum ; and since that period it has formed the subject of frequent meditation, which has at length assumed the form of the present treatise. Dr Wightman candidly acknowledges that he was not aware of any specific treatise on the subject either in the English or any other language ; and it was only after he had been far advanced in the composition of his treatise, that he became informed of the writings of Veegens and Rahn. He adds, however, that he has neither seen these nor any other works on the subject, and to no author who has written expressly upon the subject has he been indebted for a single remark therewith connected. The present treatise, therefore, may be regarded as in this respect at least entitled to the merit of originality.

While we do not regret that Dr Wightman has not consulted the writings of his predecessors on the subject, and are rather pleased to receive the performance of one who has been obliged to observe, to think, and to reason for himself, nor are we sure that the knowledge obtained from these sources would have in any degree added to the value of his own work ;—we may be permitted to say, that there are, besides the works referred to by him, several in which the subject is treated, not expressly perhaps, but in considerable fulness and detail. It is not perhaps reasonable to expect that he should have referred to the writings of Dr Willis, which, though now something antiquated, contain upon this very subject many original, just, and instructive observations,—especially in his treatise *de Morbis Convulsivis*, and in the *Pars Pathologica* of the essay *de Anima Brutorum*. The Lectures of Boerhaave also on the diseases of the nerves, delivered in the beginning of the eighteenth century, but only published in 1761 by Van Eems, contain many useful facts on this subject, and may be regarded, next to the writings of Willis, as the great source from which subsequent writers derived many of their doctrines.

We have, however, on a former occasion, (in Vol. xlv. p. 441,) spoken so fully of the writers on this subject, that it is unnecessary to do any more at present than indicate merely the names and writings of those who have studied most to elucidate the subject. The dissertation of Daniel Langhans, *De Consensu Partium*, published at Gottingen in 1749, and afterwards republished in 1757 by Haller, in the sixth volume of his Collections, illustrating the Nature and Treatment of Diseases, contains the first systematic view on this subject. Next to this may be placed the Observations of Dr Robert Whytt on Nervous Disorders, published first in 1764, and afterwards in 1765 and 1767. The work of Rahn did not appear till 1777, and was republished in 1795 by Ludwig in the fourth volume of his Collection of Writers on Neurology. All these works are excellent, and merit attentive perusal; but all of them, perhaps, are exceeded in the comprehensiveness of plan and completeness of detail by the excellent dissertation of John Heineken, published at Gottingen in 1783—*De Morbis Nervorum eorumque frequentissima ex abdomine origine*, and republished by the same collector in his second volume in 1792. The detailed mode in which Heineken traces the origin of many nervous diseases to disorder in the abdominal viscera, and the connection between the splanchnic nerves and the cerebro-spinal axis, shows that he understood the nature of this connection very completely, and well appreciated its influence. Our limits will not allow us at present to describe the course of this learned author in tracing the etiology of the multiplied diseases of the nervous system from the disorders of the abdominal viscera; but all those who wish to understand thoroughly the nature of these connections, will find that, in the dissertation of Heineken, scarcely one disease of the nervous system is omitted.

Of the work of Veegens, which is mentioned by Dr Wightman, we have only to say, that it appeared in 1784 at Leyden, and was republished by Schlegel in his Thesaurus at Leyden in 1787; and that the true orthography of the name is Veegens and not Vegens. Dr Wightman will now be enabled to understand which is right, and to reconcile the apparent discordance in the date of publication.

Four years after the appearance of the dissertation of Heineken at Gottingen, William Godfrey Ploucquet, and Christopher Ludovic Baur, published at Tubingen an able dissertation on Cephalalgia and its different species; and, while in this they enumerated about fifty different varieties of headach, according to the causes from which the disease proceeded, they also considered carefully those varieties which depend on disorder of the abdominal viscera; and, though their view of the subject is in one respect too extensive, viz. in considering headach as of so many different sorts, and in another too limited, namely, in confining their views to headach alone, without regard to its accompanying symptoms, as, vertigo, confusion, weight, or its pathological causes, it must, nevertheless, be allowed, that the dissertation of Ploucquet and Baur forms an instructive link in the chain of the literature of this subject. This dissertation was republished in the third volume of the Collection of Ludwig.

The extent of this list we might increase considerably, by referring to the writings of various authors who have treated of this subject more or less fully, if not expressly, at least along with other subjects, of which this formed part, and to various essays or sketches which have appeared during the last thirty or thirty-five years, in different journals devoted to medicine and the medical sciences. We must be satisfied, however, by referring in general to the excellent dissertation of John Frederic Lobstein of Strasbourg, on the Structure, use, and Diseases of the Great Sympathetic Nerves, published at Paris in 1823. The third section of this commentary, devoted to the pathological relations of the great sympathetic nerve, contains a short but pretty correct view of the sympathetic influence exerted by the abdominal viscera and the splanchnic nerves over the nervous system at large, and especially in throwing it into various morbid conditions.*

After these preliminary notices we now proceed to give some account of the labours of Dr Wightman on the same subject.

After some introductory observations illustrating the nature of the influence subsisting between the brain, the centre of the nervous system, and the stomach, the chief of the digestive organs, the author proceeds to offer some general observations on the sympathetic relation between the digestive and nervous systems, showing first the influence of the stomach upon the economy in general, and the nervous system in particular; and secondly, in what manner, and to what extent, the brain exerts its influence over the stomach in performing the process of digestion. To show more clearly the nature and reality of the latter influence, he refers to the experiments in which Dr Wilson Philip and M. Brachet divided the eighth pair of nerves. He then adverts to the influence of the depressing passions, as fear, anxiety, grief, &c. in impairing the action of the stomach, and consequently the general health, and the influence of the exhilarating passions in increasing the digestive powers and improving the health.

He then proceeds to the more special object of his essay by inquiring into the illustrations of this sympathetic relation in the causation and cure of various diseases. And he, in the first place, impresses strongly on the physician and surgeon the indispensable necessity of paying great attention to this relation or sympathy both in observing the causation, and in conducting the treatment of diseases.

He then considers the effects of injuries of the head in inducing disorder of the gastric functions. The following observations will show the manner in which Dr Wightman treats this part of the subject.

"Every experienced surgeon has, I doubt not, frequently met with cases in which, after external injury to the head, when all symptoms of disease in the brain have been removed, various distressing morbid affections of the stomach and the other chylopoietic organs take place, as nausea, vomiting, furred tongue, loss of appetite, deranged digestion, and constipation of the bowels. These sympathetic affections occasionally proceed to such an extent, as to resemble in an exquisite degree the symptoms of idiopathic dyspepsia, and continue to harass the patient for a considerable time. But in some cases a reaction is at length established in the brain; a relapse of the primary disorder of that organ apparently occurs, as the headach, vertigo, delirium, and other symptoms return. The unreflecting practitioner, unaccustomed to take into consideration the sympathetic origin of diseases, will find himself puzzled to account in a satisfactory manner for this change in the case; but the fact will at once be obvious to those who look not at symptoms merely, but also search for their pathological cause. It is thus explained. The stomach and bowels having been deranged sympathetically in the first instance, and unable to perform their functions properly, digestion and chylification being either much impeded, or almost interrupted, these organs are oppressed with a load of undigested aliment, becoming by the retention putrid and offensive, which thus proves a source of irritation and excitement to the brain and nervous system; and, through these, to all other parts of the animal frame. Hence the symptoms in the head already enumerated. Should any other proof be required of the truth of what I have now advanced, it is afforded by the effects of the remedies. The stomach and bowels having thus become the organs *chiefly, mainly* affected, although the morbid cause of their deranged actions was in the *first instance* in the brain; *now*, from the reaction that has taken place, the brain *in its turn* becomes the organ *secondarily* affected in the new morbid association. The exhibition of mercurial purgatives, combined with other active cathartics, by removing the noxious and irritating cause from the alimentary canal, and promoting the free secretion of bile and the intestinal fluids, will often suffice to remove all the morbid symptoms in the brain and nervous system thus induced through mere sympathy, and without any remedies directly applied to the latter. If, notwithstanding the free operation of these remedies, the morbid symptoms in the brain continue, we have ample evidence that a derangement of the functions of the stomach and bowels has, at this late period, re-excited into actual disease, the brain and its membranes, on the principle '*Ubi irritatio, ibi fluxus;*' an undue determination of blood taking place, and producing, if not counteracted, symptoms of inflammation itself, and requiring for its removal, in addition to a perseverance in the purgatives with a view to their depleting influence, the active employment of direct remedies for controlling the circulation, as general bleeding; as well as for subduing the increased impetus of the vessels within the head, as the topical abstraction of blood in the various ways above-mentioned, shaving the head, and the application thereto of refrigerating epithems, and also the exhibition of antimonials, and of mercury to produce its constitutional antiphlogistic influence. For here we have a second time to combat a severe affection of the brain induced entirely by sympathy with those organs, which, in the original train of morbid action, were only secondarily affected; thus presenting a clear and decisive proof of the soundness of the maxim which I propounded at the first, that in all cases, in whichever of these systems the primary seat of disease may be, it is of the most vital importance assiduously to attend to the state of the other."—Pp. 32–35.

---

* De Nervi Sympathetici Humani Fabrica usu et Morbis Commentatio Anatomico-Physiologico-Pathologica. Tabulis aeneis et Lithographicis illustrata, Auctore John Fred. Lobstein, Medicinæ Clinicæ et Anatomiæ Pathologicæ en Facultate Medica Argentoratensi Professore, &c. Parisiis, 1823.

These considerations lead Dr Wightman to suggest, that, in all cases in which young persons suffer from obstinate dyspeptic symptoms, it is requisite to make strict inquiries into their previous history, as it not uncommonly happens that these symptoms may be traced to the effects of a blow on the head received many years before; and if this be ascertained to be the case, it is then necessary to employ remedies suited to remove the diseased action in the head, before it can be expected that the gastric symptoms can be relieved.

Under the head of Typhous Fever, the following observations on the state of the intestinal canal, its influence on the state of the brain, and the necessity of rectifying the condition of the former by means of emetics, and by suitable and efficient laxative and eccoprotic medicine, deserve attention.

"So far as my opportunity of observation has extended, I can confidently state that I have seen the most excellent effects from emetics in the beginning of fever; and I am inclined to consider it in general a very judicious and useful mode of treatment. I am, at the same time, decidedly opposed to the practice of carrying the employment of *nauseating* medicines to that extent which was done, when the theory of fever being caused and kept up by an atony of the extreme vessels, and their consequent spasm, was prevalent among practitioners; for removing which spasm they considered it necessary to exhibit frequently repeated doses of emetics and nauseants. These I do certainly, in the most unequivocal manner, condemn. All I wish to observe is, that as in typhus the stomach and bowels are evidently loaded with putrid and offensive accumulations of sordes, the consequence of imperfect digestion, an emetic at the commencement is very often attended with the most beneficial consequences in repressing the severity of many of the symptoms, and insuring the favourable progress of the disease through its subsequent stages. And in its course, if there be much nausea and oppression of the stomach, with parched and loaded tongue, and further, as headach, delirium, and other symptoms of disorder of the brain are frequently produced by undigested matters and vitiated secretions lodged in the stomach and upper part of the intestinal canal, which have become more acrid and corrupted during the continuance of the disease—provided we can be assured that there does not exist any degree of gastric inflammation, an emetic may be occasionally useful, by removing the symptoms above-mentioned, rendering the others milder, and the subsequent progress of the malady more propitious. Bearing in mind the high probability, that the brain is the organ primarily disordered, and the important necessity I am so anxious to inculcate, of directing our most vigilant attention to that which suffers only by sympathy, I may say, in the words of Dr Clutterbuck himself, that 'in order to account for the efficacy of emetics in the cure of fever, it is only necessary to advert to the intimate relation that subsists between the brain and the stomach, and the influence exerted by each over the other reciprocally.' After the emetic practice has been judiciously premised in the cases best suited for its successful operation, then, I can, without the hazard of question, assert, that the exhibition of purgatives to the extent not merely of evacuating the bowels of all extraneous fæculent deposits, according to the views of Dr Hamilton; but also, without, however, inducing copious purging, of exciting and improving the secretions of the liver and intestines, is often attended with the effect of relieving the delirium, vertigo, headach, and other symptoms of oppression of the brain."—Pp. 43-45.

Dr Wightman, however, is not an exclusive gastric pathologist in fever. He admits the necessity of administering remedies calculated to act on the brain and its membranes.

"That the brain is often much affected in the course of the fever, the suffused eyes, the stupor, the disturbance and interruption of the functions of this organ unequivocally demonstrate; and the application of means intended to remedy directly this affection of the brain, every practitioner is imperiously called upon to prescribe. These are the abstraction of blood from the forehead and temples, and from the posterior auricular part of the cranium by leeches; the application of cold in the form of iced-water, or of refrigerating lotions and epithems to the shaved scalp; and of blisters either to the scalp itself or to the nucha, according to the state of the symptoms and the stage of the disease. That the prudent application of remedies such as these, suited to the removal of the diseased state of the brain and nervous system, has also a great effect in amending the condition of the stomach and bowels, is as certain on the one hand; as, on the other, that the removal of undigested aliment from the stomach by emetics, and of accumulated fæcal deposits from the bowels by purgatives, will not only operate powerfully and beneficially on the stomach, mouth, and tongue, relieving thirst, restoring and improving the secretions of the liver and intestinal canal, and thus ameliorating the state of the whole system of the digestive organs; but will also alleviate the headach, the vertigo, the confusion, and delirium, and the other symptoms of oppression of the brain, to a degree that will scarcely be credited by those who have not followed this practice; thus affording a clear and ample demonstration of the great influence exercised by the stomach and other digestive organs over the brain and nervous system —and, conversely, in the cure as well as in the causation of fever."—Pp. 47-48.

He then concludes the subject with the following remarks, which we regard as entitled to the serious attention of many practitioners, as we believe that the mistake alluded to is the cause of many relapses, and not a few fatal terminations in fever.

> " Before concluding the consideration of this sympathy in the treatment of fever, I must observe, how necessary it is to guard, during the early stage of convalescence, a period when the nervous system is peculiarly susceptible, against all irritation of the stomach from too great a quantity, or from the improper quality of food. By any error of this kind sympathetic disorder of the nervous system is very apt to be excited, and sometimes this proceeds so far as to cause a relapse of the fever, more dangerous, indeed, than the disease in its first career; as, by reason of the debility that has already been induced, the body is unable to endure a repetition of those measures, which were successful in subduing the morbid affections following the primary attack."—Pp. 49–50.

Under the head of Apoplexy, we find some judicious observations on the gastric apoplexy of Selle and Stoll, and the nervous apoplexy of Dr Kirkland.

> " It has, however, by no means been proved, that such a disease as the nervous apoplexy really exists; and I apprehend that in some of the cases, of which Dr Kirkland has seen the fatal result, it would have been found upon examination, that pressure on the brain had existed, from the extravasation of blood or serum. His description of the disease in question is a purely general one, based, it would appear, on the conceptions he had formed from his own experience; no cases are related in detail, and consequently there is no account of lesions, which might have been displayed on anatomical inspection. It is well known, that in many cases of apoplexy, whereof the subjects have exhibited the infallible characteristics of the disease, as well as of the previous tendency thereto, no abnormal appearances, after the most careful examination, have been found to account for death, such a state being designated by Dr Abercrombie as that of simple apoplexy. It should also be remarked, that in certain instances the face is absolutely pale, sometimes even of a cadaverous aspect, (doubtless owing to the accumulation of blood in the internal vessels of the head,) in which relief has been obtained as soon as blood has been got to flow from venesection; the symptoms of pressure on the brain gradually diminishing, and the paleness of the countenance giving place to the natural colour. Moreover, in some fatal cases of the latter description, distension of the vessels of the brain has been perceived, also the rupture of these, and the consequent extravasation of blood. We must, therefore, take with much latitude the assertion of Dr Kirkland, because, if such a doctrine obtained credence, the most disastrous results would frequently take place,—where they might have been prevented—from the omission of that powerful remedy, the abstraction of blood; and from the substitution, in its stead, of the use of emetics, a mea-

sure tending to produce great injury, from their effects in propelling the blood with too much force to the head.

"Besides the vehement form of the nervous apoplexy, asserted by Dr Kirkland to have been fatal in every instance, he has described a milder form, which, he says, although not very easily, he has yet known to be cured in several instances; and that such milder form is of that kind, which very often leaves a hemiplegia behind it. Now, the very circumstance of hemiplegia having taken place, is quite conclusive in my mind as to the existence of pressure upon the brain; and in any case, where there has been hemiplegia at a former period of life, if, upon an attack of apoplexy, the treatment be conducted on the principle, that the symptoms depended upon merely an irritated condition of the nerves of the stomach, it would lead to fatal errors in practice, both of commission and omission; of the former, I need scarcely repeat, by the exhibition of emetics; of the latter, by leaving the abstraction of blood undone."—Pp. 55—57.

Dr Wightman is here inclined to deny the existence of nervous apoplexy, as described by Dr Kirkland; *first*, because that author has made concerning it only general statements, unconfirmed by any particular facts; and *secondly*, because, while the vehement form of the disease is always fatal, the mild form is represented very often to leave behind it hemiplegia,—a circumstance which, says Dr Wightman, is quite conclusive as to the existence of pressure on the brain.

We confess that we have always doubted the existence of this nervous apoplexy of Dr Kirkland, that is, we have not seen reason to admit such a form of apoplexy as he describes, and, as he represents, unconnected with some lesion in the brain or its membranes. The truth is, that the gastric and the nervous apoplexy are most probably to be referred to the head of the meningeal apoplexy of M. Serres, without extravasation, which is known to be quite adequate to the production of all the symptoms, or to softening of small portions of the brain, which may have been overlooked. We believe that there are occasional instances of somnolence, and even coma, simulating apoplexy, dependent on repletion or other disorder of the stomach, and disappearing either on the supervention of spontaneous vomiting, or on the evacuation of the stomach by an emetic. But this must not be regarded as apoplexy either in its remote or its pathological cause.

Dr Wightman is then led to consider the propriety of a method of treatment sometimes adopted in apoplexy, viz. the administration of emetics. The exhibition of these remedies, though recommended by Sydenham and Pitcairn, and afterwards by Fothergill and Kirkland, Dr Wightman very justly regards as a practice of ambiguous, if not of pernicious effect. He thinks, that even in cases in which the apoplectic symptoms ensue upon distension of the stomach by an excessive quantity of food and drink, though the use of the emetic is admissible, yet it will be safer to procure full blood-letting, than to trust to the doubtful effects of vomiting.

Though Dr Wightman, however, does not admit that apoplexy, properly so called, originates in the stomach, he allows that gastric irritation, overdistension, and other morbid states of the alimentary canal, may induce conditions of loss of sensation, consciousness, and motion, so similar in all respects to apoplexy, as to be with great difficulty distinguished from that disease; and he cites in illustration a passage from the work of Dr Stone, and various cases from Dr Wood of Philadelphia, and other observers. The writings of physicians, indeed, contain various cases of this kind; and we have little doubt that these cases have afforded the ground for establishing the variety called gastric and nervous apoplexy, and have also furnished arguments against the use of blood-letting, and in favour of the use of emetics.

The author next considers the connection between gastric disorder and epilepsy; and the following extract may be given as presenting a sort of abstract of his views:

"The affection of the brain in this form of epilepsy, although sympathetic in the first instance, at length, by frequent repetition of the paroxysms exciting irritation in this organ, becomes real; and a flow of blood then takes place towards it, requiring for its removal, depletion by general bleeding, leeching, or cupping. If the symptoms of the paroxysms be very severe, this will, on their invasion, be urgently demanded; but if, in the intervals, the patient be subject to indications of vascular fulness of the brain, as stupor, drowsiness, dilatation of the pupils, vertigo, starting during sleep, the repetition of these measures will be required from time to time. But, if the more urgent symptoms have been mitigated, all that will be afterwards necessary may be accomplished by repeated leeching on the temples; or, as more efficacious, on the internal nasal membrane lining the septum, or on the space behind the ears. The other auxiliary measures for the relief of vascular cerebral fulness must also be had recourse to, such as the application of cold, an elevated position of the head, the excitement of warmth in the feet, and of a gentle determination to the surface; but above all, as derivative, the free evacuation of the serous portion of the blood, by powerful cathartics.

"In attacks of this disease on their first appearance and early repetitions, great caution, as in all other cases of determination to the head, must be observed in the use of emetics. These are never, without much consideration and circumspection, to be exhibited. If the disease has been evidently caused by certain indigestible or poisonous substances taken into the stomach, then they may not only be safely administered, but they are imperatively called for. When the malady recurs after frequent repetition, provided there are no symptoms of vascular cerebral plethora, but in persons of an atonic constitution, marked by a pallid aspect and flaccid fibre, and if it should be obvious, on proper investigation, that the recurrence of the fits is promoted by the force of habit; and if there be premonitory symptoms of continuance sufficiently long to enable us to exhibit emetics; then their use may be beneficial, by arresting the paroxysm, and thus breaking the force of the habit. If the patient has been addicted to a gross mode of living, and from this cause is subject to symptoms of dyspepsia, in such case even in the intervals, their occasional use to evacuate the stomach, and also to change the habit, may be advantageous. If emetics should be exhibited, on account of indications presented by the state of the stomach, in any case where there are signs of sanguineous determination to the head, the vascular fulness must always, in the first instance, be relieved by bleeding, general and topical, and the free administration of purgatives, according to the degree in which this fulness exists.

" There is a certain class of cases of epilepsy, wherein much benefit has been obtained from the exhibition of the oil of turpentine; which cases may be adduced as affording special and satisfactory evidence of the sympathetic relation between the digestive organs and the brain; as these originate in a morbid state of the alimentary canal, which has been caused by depraved secretions, or other noxious matters, irritating the nervous fabric distributed upon its mucous membrane. The beneficial effects of this remedy are, no doubt, partly to be attributed to its action as a purgative. This operation, however, may generally be more certainly accomplished, by other cathartics combined with calomel, or by a dose of calomel exhibited previously to a purgative dose of the oil of turpentine. But the efficacy of the medicine, when administered in smaller and repeated doses, is to be ascribed to its benign, its composing influence on the irritated nervous fabric of the intestinal canal. I can, unequivocally, bear my testimony to the accuracy of Dr Prichard's observation, that it very soon materially changes the state of the mucous membrane of the alimentary canal, occasioning moderate and regular evacuations, correcting the tendency to a frequent repetition of griping and irritating stools; at the same time exerting a peculiar sedative or tranquillizing power over the nervous system, lessening irritability, the disposition to starting and convulsive twitching of the muscular fibres, and promoting sleep; this curative power of the remedy being, as as well as the irritation which it removes, effected through the sympathy of the brain and nervous system with the digestive organs; and thus the influence of their reciprocal action is proved, both in the cure and in the causation of this disease. When the malady is either originally idiopathic; or the morbid action, by frequent repetition, has been altogether removed from the intestinal canal to the brain, and the disease in this secondary way has become purely cerebral, just as if it had been originally so; the oil of turpentine can exert no beneficial influence upon it. In these instances, there can be no objection to a trial of the remedy; but if, after sufficient perseverance in it, the paroxysms continue unmitigated in severity, and recurring as frequently as before, there are then good grounds for believing that the brain has been seriously involved in the morbid action, and that the disease is of that character, in which this medicine can have no good effect whatever, and that therefore it is worse than useless further to persevere in it.

"In the epilepsy sympathetic of intestinal disorder, after the morbid cause has been removed by a judicious exhibition of carefully selected and combined cathartics, and, if deemed proper, by the occasional use of emetics, the employment of those medicines known by the designation of *Antispasmodic* and *Nervine,* such as the fœtid gums, camphor and valerian, simultaneously with tonics, as cinchona, either in substance or in the form of the disulphate of quina, and the sesquioxide of iron, will often prove highly advantageous. Some persons are disposed to underrate the value of such antispasmodic or nervine medicines; but, when cases have been found to yield after their exhibition, which had previously resisted other remedial treatment, there is surely reason to conclude favourably of their operation in subduing the disease. This operation is doubtless to be explained, on the principle of their influence on the nervous system, communicated thereto from the primary impression made upon the nerves of the digestive organs."—Pp. 83—88.

In considering the indications of this sympathy between the brain and stomach, as presented by the disease denominated hydrocephalus, Dr Wightman partly adopts and partly modifies the views entertained of the original source of that disease by Dr Hamilton and Dr Cheyne. Dr Hamilton had often observed it ensue on the intestinal disorder which he describes under the name of marasmus; and every observing and experienced physician can bear testimony to the accuracy of this observation. It is only to be remarked, that the form of the disease which seems to ensue on marasmus is more tedious and chronic, and less marked in its symptoms, than the usual form of meningeal or hydrocephalic inflammation in infants and children. It may also be added, that it is often a matter of some doubt whether the intestinal disorder, marked as it is by slow torpid bowels, obstinate constipation, puffy swelling of the belly, foul breath, dry tongue, and restless nights, be the cause or the effect of the meningeal disorder. Dr Cheyne placed the primary symptoms chiefly in disorder of the liver, the circulation and secretions of which, he argued, were very much deranged.

Dr Wightman thinks that Dr Cheyne ascribed too much to the morbid state of the hepatic system, and too little to that of the stomach and bowels; and he adduces, in proof of the justice of the idea, that the functions of the latter organs are greatly disturbed, the vomiting, the loss of appetite, the coated tongue, the sickly fetid breath, the thirst, the constipation and torpor of the bowels, and the unhealthy nature of the alvine evacuations.

"All these symptoms indicate, certainly, disorder in the *primæ viæ;* but, whether they are to be attributed to primary disease in these, or to be considered sympathetic of a morbid condition of the brain, is often a question of most difficult solution, especially as in such cases, intense headach and intolerable nausea by turns harass the sufferer; and, while the nausea continues, the patient occasionally complains of pain in the region of the stomach. The irritability of the stomach is often so great, that retching and vomiting are induced by any changes of posture, the slightest movement of the head, and almost always by an attempt to sit upright in bed."—P. 93.

In short, Dr Wightman thinks that there are two modes in which the phenomena presented by the sympathy in hydrocephalus may be produced; *first*, by the occurrence of diseased action in the stomach and bowels, and then the supervention of disorder in the brain by irritation propagated from the stomach to that organ; and *secondly*, by the occurrence of disease in the brain and nervous system from other causes, but inducing there a primary disease, which subsequently gives rise to disorder in the chylopoietic organs.

Dr Wightman then adverts to the opinions of those physicians who, in imitation of M. Broussais, ascribe all irritation in the brain to previous irritation in the gastro-enteric mucous membrane, and accuse, as causes of this irritation, the exhibition of purgative remedies, so generally used by English physicians, and so much condemned by foreign physicians and their followers. The following observations are suggested by the doctrines of this class of physicians.

"Far indeed am I from opposing the practice of leeching the epigastrium, where gastro-enteric inflammation is *undoubtedly ascertained to exist.* But, to persevere in this course, on the mere supposition of the existence of this morbid state, there is too much reason to believe, has often led to the injury, and sometimes the death, of the tender sufferers. So considerate truly ought we to be, before adopting the notion of gastro-enteritis, that, if we find a child labouring under either constant irrepressible vomiting, or occasional vomiting with sickness in the intervals, which has been recurring day after day, or even with intermissions of one or two days; we should always put this question to ourselves, and to those who may be associated with us in attendance, *whether it does not depend upon disease of the brain;* for it ought then to be well recollected, since the stomach by its intercourse with the brain affects so many organs, how readily it may, through the same intervention, be affected by disease in other organs; but far more remarkably by *disease of* *the brain itself*, with which it *so peculiarly, so directly sympathizes.*

"The symptoms of disorder of the *primæ viæ*, which, in addition to others, gradually precede the supervention of disease in the brain, are, impaired and capricious appetite, white or loaded tongue, constipated or variable state of bowels, and the altered appearance of the dejections. If these symptoms have been overlooked, or have not existed in any case to which we are called, and in which vomiting with or without headach is a prominent symptom, having resisted all the usual means, employed on the supposition of its gastric origin, to repress it, we then have reason to conclude, that disease in the brain, whether idiopathic or symptomatic, is *established*; and the vomiting must be regarded *not* as the indication of a primary affection of the stomach, but as symptomatic of cerebral disease; just as this symptom is occasioned by contusions and injuries of the head; and further, we have reason to conclude, that this specific encephalic inflammation is the cause, which has produced all the symptoms so pertinaciously ascribed by the Broussaian disciples to the persistence of gastro-enteritis; symptoms which, instead of leeches to the epigastrium, demand their application to the temples and the space behind the ears; blisters to the nape of the neck; evaporating lotions or cold epithems to the head, or the direction of a stream of cold water thereupon; the exhibition of purgatives with all due care to ensure, if possible, their retention upon the stomach; a warm pediluvium, rendered irritating and derivative by the addition of mustard; the speedy influence of the mercurial action, with the proper elevation of the head, and the seclusion of the patient in quietude and darkness. I shall not apologize for repeating, because it is a most important remark, that we shall never take a proper pathological view of any case, and, of course, can never establish a rational method of treatment, while an undue preference is given to either the brain and nervous system on the one hand, or the stomach and its subservient viscera on the other; while the remarkable sympathy existing between the two systems is not fairly taken into account, in weighing all the symptoms in every case. If Dr Yeats has too exclusively viewed the disease as originating in the *primæ viæ*, he has qualified the doctrine in no inconsiderable degree, by stating his opinion, that, whether there is a conversion of disease from the digestive organs to the brain, or a synchronous morbid action existing between them, the *brain should be an object of watchful practice*, without neglecting to pay particular attention to the former. Although, however, this excellent physician is so strongly inclined to the belief, that, in the great majority of examples of this disease, it is symptomatic of primary disorder of the chylopoietic organs, he does not deny—*that* we must do him the justice to say—the occasional occurrence of the idiopathic disease, independent of previous disorder of any other organ."—Pp. 99—102.

The author next applies his views to worm fever, and the infantile remittent fever, both of which, it is well-known, present symptoms of cerebro-meningeal irritation, so similar to those of hydrocephalus, that authors have laboured to give a correct diagnosis between them.

Dr Wightman considers successively the subjects of chronic disease of the brain; headach of different kinds; dyspeptic and nervous disorders, combined with the influence of mental labour; the operation of certain poisons, especially of the narcotic tribe; and concludes with some remarks on the recent censures pronounced on the doctrines and practice of the late Dr Hamilton.

On the last topic we have more than once had occasion to speak; and we are happy to see that Dr Wightman takes the same view of the matter as we have done. With regard to the alleged indiscriminate employment of purgative medicines in the practice of Dr Hamilton, and its supposed fatal effects, mentioned in one of our contemporaries, we must say that, after observing his practice for several years, we never witnessed it, nor could we see any reason for ascribing the deaths among his patients to this alleged indiscriminate employment of these medicines. On the contrary, we feel it to be a sacred duty to the memory of a most skilful and judicious physician, to say that we have often beheld with agreeable surprise, the extraordinary recoveries which took place under his treatment.

The appendix contains a warm panegyric on the method recommended by Dr Wilson Philip, of treating chronic diseases by minute doses of mercury; an exhortation on the necessity of perseverance in remedial measures for the successful treatment of chronic disease; and some observations on the use of tonics in the removal of the debility attendant on chronic disease.

We are surprised, nevertheless, that Dr Wightman has not adverted to St Vitus's Dance, nervous paralysis *(Paresis)*, spinal irritation, and certain forms of hysteria, in the production of all of which, the existence and influence of this sympathetic connection between the alimentary canal and the nervous system are not less clearly evinced than in the disorders considered in the volume.

In conclusion, this small volume contains a considerable amount of useful information, not perhaps altogether new, but not the less important on that account. The author has bestowed much study in illustrating his subject; and his work furnishes in a small compass an excellent practical view of a very important class of principles in pathology and therapeutics. It is, indeed, the only work in the English language which gives a generalized view of all the isolated facts and doctrines in this department of pathology, and in that respect deserves the attentive perusal of the intelligent medical practitioner.

ART. VII.—*The Retrospect of Practical Medicine and Surgery for the year* 1840. *Being an Analysis of the British and Foreign Medical Journals and Transactions; or a Selection of the latest Discoveries and most Practical Observations on the Practice of Medicine, Surgery, and the Collateral Sciences for the past year, made chiefly with reference to the Treatment of Disease.* By W. BRAITHWAITE, Member of the Royal College of Surgeons, &c. Vol. I. No. 1, January—July. No. 2, July—December. London, 1840. 12mo, Pp. 200.

THE number of periodical publications devoted to medicine, surgery, and the collateral sciences is at present so great that it requires no ordinary diligence to read or even glance at all of them; and, perhaps, few persons engaged in the active duties of the profession have leisure to do so very carefully. Though all the periodical journals profess to give the substance of new works and papers published from time to time, yet sometimes the accounts of the contents of these new papers and writings are necessarily more lengthened than suits the time of many readers; and while these accounts contain principles and reasonings, often the mere practical application is the point which interests the practical reader.

Impressed with these circumstances, Mr Braithwaite has undertaken to do for the public what he previously found useful to himself, namely, to select from the whole of the journals and periodical publications, in a short and abridged form, such pieces of information on the practical or applied departments of medicine and surgery, chiefly, in other words, the therapeutical department,—as might seem most important and necessary to be known by the practitioner. It is manifest, that, while this undertaking requires considerable knowledge and judgment, it is also one which, if well done, cannot fail to abridge and facilitate the labour of the occupied practitioner.

The task which he has prescribed for himself, Mr Braithwaite has performed very creditably. He has condensed in about 400 duodecimo pages, the essential part of about twelve medical periodical publications. So far as we can perceive, he has preserved as much of the important and useful matter as the limits of his publication will allow. A few remarks of his own he has in some instances added; but, in general, he has exercised this part of his function cautiously; and, perhaps, it is a privilege which requires to be cautiously used.

The publication of Mr Braithwaite deserves the attention of the practitioner; and we hope it will receive proper encouragement from the profession.

PART III.

MEDICAL INTELLIGENCE.

PHYSIOLOGY.

Experiments on animals with the view of ascertaining the effect of Ligature of the Carotid Arteries. By M. Jobert of Lamballe. (*Bulletin de l'Academie Royale de Medecine,* Oct. 1840.) These experiments were undertaken with the view of ascertaining the correctness of the statements of M. Mayer, regarding ligature of the common carotid arteries. M. Mayer found that ligature of these vessels in dogs, goats, sheep, horses, and rabbits, was followed by serious disturbance of the cerebral and respiratory functions, with loss of lustre, inflammation, or suppuration of the eyes, if the animal lived long enough; but in all death followed after a longer or shorter time.

M. Jobert first tied both the carotids of a female rabbit; but no function of the animal was observed to be disturbed. It was killed seventy-five days after the operation. The arteries below the ligatures were reduced to the state of ligamentous cords. The vertebral arteries had acquired a volume greater than ordinary, but the cranial and ophthalmic arteries were normal.

Eight times M. Jobert tied both the carotids of the horse with the following results. In the first horse the pneumogastric nerve was cut, and instantly the animal was seized with great dyspnœa, and the respiration was accompanied with a blowing sound; it fell several times, and died within an hour. In the second horse, the recurrent nerve was cut, and the animal died in two hours, during all which time the respiration was loud and noisy. In the third horse, the right pneumogastric nerve was included in the ligature, and the animal presented the same symptoms as the two preceding, and died in an hour and a-half after the operation. In the fourth horse, the nerves being avoided with care, no nervous symptoms were manifested. The respiration, however, became slower and slower, but without any unusual bruit, and the animal died in six hours. The fifth horse exhibited strong contractions of the hinder extremities at the end of three hours, which caused it to fall down. It lived seven hours. The sixth horse lived four days. The seventh and eighth horses were previously reduced by a considerable abstraction of blood, and the one died fourteen hours, the other ten hours after the operation.

All these animals were opened, but in none was the brain found in the least disorganized. In all, however, the lungs were very much congested with blood, forming, in fact, true reservoirs, to a less extent, however, in those which had been weakened by a previous loss of blood.

M. Jobert also tied the two common carotids of a sheep, but no hurtful consequences ensued. The animal was killed twelve days after. He operated also on two small dogs, which were killed after twenty-one days; no function having been disturbed in consequence of the operation during all that period. A similar result followed the ligature of these vessels on two large dogs. Dissection of these animals showed that the carotids had increased in size below the ligature; that these vessels were obliterated for a short space where they had been tied; and that the vertebral arteries had acquired the apparent bulk of the common carotid.

From these experiments, M. Jobert concluded that ligature of the common carotids did not involve any lesion of the cerebral functions, but that all the disturbances took place in the pulmonary apparatus: and that death is the result of the lung becoming fully congested with blood in its whole extent, and as it were, transformed into a reservoir of blood, as seen in the horse.

A commission of the Royal Academy of Medicine, appointed to ascertain the correctness of M. Jobert's statements, repeated these experiments on mules, horses, dogs, rabbits, and sheep, and arrived at the very same conclusions. They, as well as M. Jobert, attributed the congestion in the lungs of the horse, and consequently rapid death of that animal, to the anatomical fact, that their vertebral arteries suddenly diminish to an almost filiform calibre, so that they are unable of themselves to keep up the cerebral circulation, or allow of such a free circulation of blood as to prevent pulmonary congestion.

On the varying proportion of some of the principles of the Blood in Disease. By MM. Andral and Gavarret. (*Journal de Pharmacie*, September 1840.)—MM. Andral and Gavarret experimented on the blood of 200 patients, or 360 different blood-lettings; and the process followed in their analyses was the same as that used by MM. Prevost and Dumas. They ascertained that, in disease, taking 1000 parts of blood, the fibrine may vary in proportion from 1 to 10 in that quantity; the globules from 21 to 185; the solid matters of the serum from 57 to 104; and the water from 725 to 9151; but it was very rare to find these different ingredients increased or diminished simultaneously. Sometimes only one ingredient was changed in proportion, whether it was increased or diminished; sometimes two were altered, but generally in an inverse ratio, as, for instance, if the fibrine were increased, the globules were diminished, and *vice versa*. From this remarkable circumstance MM. Andral and Gavarret divide into four classes the diseases which produce an alteration in the composition of the blood.

The first class comprehends those diseases in which the fibrine is constantly augmented; such are the phlegmasiæ.

The second class comprehends those diseases in which the fibrine is never increased in proportion, but often diminished; such are the pyrexiæ.

The third class includes those diseases in which there is constant diminution of the blood globules, as in chlorosis, &c.

In the fourth class are included those diseases or morbid states in which the albuminous matter of the blood is diminished, as in Bright's disease of the kidney, &c.

But this simplicity of arrangement cannot always be observed; for it often happens that many morbid states, each of which produces its peculiar alteration in the blood, are met with in the same individual. In this case they were able to trace distinctly in the blood the indications of this complication.

Besides disease, losses of blood and low diet were found to have a powerfully modifying effect on the relative proportion of the ingredients of the circulating fluid, and somewhat masked, as it were, the peculiar alteration of the blood caused by disease. They chiefly acted, however, on the blood-globules which they diminished; but it was found that, from one blood-letting to another, the globules were not diminished in the same proportion in all patients. In this respect there were very great differences in different individuals; so much so, indeed, that whilst one would between each bleeding lose only from 2 to 3 parts in the 1000, another would lose from 30 to 40.

At the same time that the blood-globules diminished from blood-letting, the fibrine most generally maintained its original proportion, rarely diminished, but sometimes increased.

When the disease was of that nature that the increase of the fibrine was one of its necessary results, this increase took place in spite of blood-letting and diminution of the blood globules. In order that loss of blood should effect the diminution of the proportion of the fibrine, it was necessary that it should be carried to a very considerable extent. It was then found that all the solid ingredients of the blood diminished simultaneously.

On the Existence of Fibrine in the Blood in the shape of Globules. By Mr ADDISON. (*London Medical Gazette,* 18th December 1840.)—Since the time of Hewson, it has been the commonly received opinion, that fibrine existed in a state of solution in the serum of the blood. He arrived at this conclusion from observing, that, when a buffy coat was forming on the surface of recently drawn blood, the clear serous-looking fluid collected from its surface with a spoon separated into serum and a fibrinous clot. Subsequent observers having remarked the same fact, arrived at the same conclusion; and it is not a little strange, that, whilst other portions of the blood have been frequently subjected to microscopical examination, this portion, (viz. the serous-looking fluid collected from the surface of recently drawn blood, and which forms the buffy coat,) has been passed over unnoticed. A few observers have, it is true, observed colourless globules in the mass of the blood, mixed with the coloured globules; but their true nature has never been hinted at.

Mr Addison, on placing in the focus of a microscope, a drop of the serous fluid which collects on the surface of the clot, found it to consist of serous fluid, and " an immense multitude of clear colourless globules." Some of the same serous fluid, removed from the surface of the recent blood before coagulation had taken place, separated into serum and a clot of fibrine. This experiment was frequently repeated, and always with the same result.

It is astonishing that analogy did not lead physiologists or anatomists long ago to this very result; for, on examining any portion of fibrine with the microscope, each fibrilla is always found to present the appearance of a congeries or string of globules. This discovery will also explain many circumstances connected with the physiology of the blood, hitherto not properly accounted for. It will go far to explain the appearance of the buffy coat on blood drawn from a vein, as the different specific gravities of the fibrinous and red globules, in particular conditions of the blood, may account for the greater or lesser thickness of fibrinous layer on the surface.

On the Contractility of the Blood-vessels. By Dr HENLE of Berlin. (*Wochenschrift fur die Gesammte Heilkunde.* October 1840.)—Dr Henle endeavours to prove that all congestion of blood in the capillary vessels, and inflammations, are due to a species of paralysis of the coats of the capillary vessels. To prove this fact, he first endeavours to demonstrate that these vessels are possessed of a certain degree of contractility in their normal state, and that they are under the influence of the nervous system, as well as all other parts which contain muscular fibres. He then shows that they possess the same structure as the blood-vessels of greater calibre, and that these last, as well as the solid or hollow muscles, whether of organic or animal life, are composed of the same elements. This analogy of function and of structure, he appears to have proved from the microscopical examination of the muscles, the muscular tissue of the heart, the middle coat of the arteries, the walls of the capillary vessels, the subcutaneous cellular tissue, and the dartos. The functional differences of these structures, he says, consist in this, that the movements of the solid muscles are subject to the will, and may be determined by mechanical and galvanic irritants, but not by the action of cold; whilst the hollow muscles of organic life are excitable by the same agents as the muscles of animal life, but are incapable of being acted on by the will.

The middle coat of the arteries, which has the same structure as the muscles of organic life, cannot be excited by the will; but may by mechanical irritants and by cold, but not by galvanism. If then, says he, the circulation is carried on in the larger vessels with the aid of contractility, which is a function under the influence of the nervous system, the capillaries must act in the same way, as their structure is identical. When the contractility of the capillaries is diminished by paralysis or want of nervous energy, the circulation is badly maintained, and congestions and inflammations are the result.

M. Henle states his ideas of the proper division of inflammations and congestions of blood: but they appear not to be supported by facts.

On the Functions of the Roots of the Spinal Nerves. By M. Longet. (*Comptes Rendus des Seances de l'Academie des Sciences.*)—M. Longet has performed a number of experiments corroborative of those of Sir Charles Bell as to the functions of the roots of the spinal nerves. He repeated his experiments on seventeen dogs, exposing ten pair of roots of the spinal nerves on each dog, and in all of them it was found that the anterior roots, and the corresponding parts of the spinal marrow, were not affected by mechanical irritants of any kind, whilst the posterior roots, and the corresponding medullary cord, were extremely sensible to all these irritants.

When the two poles of a galvanic pile of twenty plates were applied to the anterior roots of the spinal nerves, violent convulsions were excited; but not a trace of convulsions was ever experienced when the galvanic influence was applied to the posterior roots. In all his experiments, M. Longet separated the two orders of roots by means of a plate of glass.

The result of his experiments clearly demonstrated that the anterior roots are exclusively the nerves of motion, and the posterior those of sensation.

That the physical and moral qualities observed in adult age are evinced at Birth. By M. Blanchet. (*Bulletin de l'Academie Royale de Medecine*, 15th Nov. 1840).—M. Blanchet, from a long series of experiments and observations extended over twenty-five years, has arrived at the conclusion, that the future size, character, &c. of an individual may be pretty accurately guessed at a few days after birth.

His first series of observations had reference to the size of the individual. He marked the heights of 100 new-born children, born between the years 1805 and 1810, and divided them into three classes according to their height. 1. Those who were shortest, and whose height did not exceed 16 inches 6 lines, twenty-five in number. 2. Those of middle stature, sixty in number, whose height did not average more than 18 inches 6 lines. 3. Those who were tallest, fifteen in number, whose height reached 20 inches 6 lines. After a period of twenty years the survivors of these 100 individuals, seventy-five in number, were again measured, and, with the exception of seven individuals, they were found to hold to each other the same relative stature. Thus, those of the first class had attained a height of only 4 feet 11 inches to 5 feet. Those of the second class, that of 5 feet 3 inches to 5 feet 4 inches. Those of the third, 5 feet 6 inches to 5 feet 7 inches, French measure. Of the seven exceptions, one had passed from the first to the second class, one from the first to the third, and one from the third to the first. The other four were out of the way, and their stature could not be ascertained.

His next series of experiments were on the development of the brain, as connected with the intellect of the individuals. But we refrain from giving any detail of these, as they take for granted certain phrenological doctrines which do not appear to be proved. He embodies his conclusions in the following words:

1. That the male infant, born at the full period, bears the impress of the character, inclination, and intellect which it will have at 25 years of age.

2. That the quantity and quality of the intellect (esprit) depends on the volume of the brain; but is influenced much by the greater or lesser developement of its anterior or posterior parts.

3. That in 100 men, 13 are born stupid, 2 idiots, 7 with a good share of intellectual endowments, 2 men of genius, 1 disposed for deeds of cruelty, 2 with superior intellectual endowments, 22 with a greater share of spirit than ordinary, and 51 with good common sense.

On the origin and mode of Development of Spermatic Animalculæ. By M. Lallemand. (*Comptes Rendus des Seances de l'Academie des Sciences*, 9th November 1840.) M. Lallemand has arrived at some very curious results in consequence of his microscopical researches on the seminal fluid of man, both in a state of health and of disease, and on that of the mammalia, birds, reptiles, fishes, crustacea, and mollusca. These experiments, at first sight leading to very different results, so illustrate each other, that M. Lallemand has been able to point out that the different pathological conditions of this fluid in man are observed in all these animals. The following are the most curious of the results at which he has arrived.

The spermatic animalculæ are the result of secretion, and like other secreted matters, are susceptible of numerous modifications, and are under the influence of all disturbing causes. Thus they diminish in number, in volume, in density, and in form, during disease; and their modifications in this respect are greater, and their resistance to decomposition less, the more serious the nature of the disease. Sometimes they become very scarce, or are even altogether replaced by pyriform, ovoid, or spherical bodies. These bodies appear to be endowed with life, for they move about with great ease, and after death assume a brilliant lustre. Their form is in general very regular. At the period of puberty these globules precede the appearance of the perfect spermatic animalculæ, and again replace them in old age, and during many diseases. They are, in fact, the results of an incomplete or imperfect secretion.

These alterations in form, size, &c. were also observed to take place in animals at their different breeding or rutting seasons, and were especially remarked in birds. In them, the testicles, which are pale, soft, and flabby during the period of repose, become injected, hardened, and reddened during the breeding season. They at first contain only a homogeneous and transparent fluid, but which soon becomes filled with granular and then globular bodies. The spermatic animalculæ soon after make their appearance, at first exhibiting but little motion; but afterwards all those signs of life usually observed when fully developed. The testicle is then four or five times more voluminous than it was before the sexual period arrived; and the very same phenomena are observed at each sexual period.

In man, it is only at the period of puberty that these phenomena are observed, excepting always when they result from the effects of disease.

In the seminal canals the spermatic animalculæ are very numerous, and clustered together in groups, and are most generally quite dry. In all these groups, the heads of the animalcules are directed towards the epididymis, whilst their tails point to the surface of the testicle. When they arrive at the *vas deferens*, they separate and become more mobile. The fluid of the *vesiculæ seminales*, of the glands of Cowper, and of the prostate, appeared to have no other use than to allow of their moving more easily. In the mollusca, the spermatic animalculæ die before the individual. This was especially remarked to occur in the bivalves, which can live for some time out of the water by keeping their valves closed. This fact is easily ascertained by examining the spermatic fluid immediately after these animals are first taken out of the water, and again before the death of the animal.

These facts appear to authorize the conclusion that spermatic animalculæ are true secretions.

Physiologists show much unwillingness to admit that glands can secrete living bodies; and that, if it were so, the testicle would be the only example. But the ovaries secrete the ovules, which, even before impregnation, are living bodies, which become more perfect in the oviducts after their separation from the ovary, in the same way as the spermatic animalculæ do after their secretion. The ovaries are precisely analogous to the testicle, so much so, indeed, that, in the inferior classes of animals, it is very difficult to distinguish them from each other. Their position, their colour, the disposition of the oviducts, and of the *vasa deferentia*, are quite similar. This similarity has caused many errors of classification, into which even distinguished naturalists have fallen. But the microscope, by detecting a spermatic animalcule in a body, which was generally considered to be an ovary, has corrected many of these errors. Thus Prevost discovered that muscles are not hermaphrodite; and Milne-Edwards and Lallemand have ascertained the same with regard to the genera Cypris and Patella. In the Medusæ, the similarity between the spermatic animalcules and the ovules is very striking, seeing that the animalculæ are enveloped in true capsules, which may be drawn out from the testicle. In some of the aggregated polypi, the resemblance is very great between the male and female organs. In them, the spermatic animalculæ are found between the digestive canal and the walls of the body, having no special organ of secretion, and the ovules are found in the same situation, there being in them no ovary.

In birds, each group of spermatic animalculæ is half-enveloped in a hollow, very thin, and transparent sac, which receives all the heads of that group.

In the crab, the straight canals which represent the testicles contain free spermatic animalculæ; but in the dilated portions of these canals, they are found enclosed in capsules with thin walls. The testicles of the cephalopoda are of a very remarkable construction. There exists in their interior a membrane which is spiral in the *Sepia octopus*, but divided into four distinct compartments in the *Sepia officinalis*. The *vas deferens* is besides very long, and spirally twisted; the glans is complex and fleshy. These organs are filled with a great quantity of a viscid fluid. Within the testicles spermatic animalculæ are found exactly similar to the ova; a circumstance which proves that these animalculæ do not come from the capsules. The formation of the spermatophora appears to be easily explicable. When the spermatic animalculæ arrive at the *vas deferens*, they come in contact with the surrounding viscous fluid, which forms a capsule or envelope over them, simple when the organs are simple, double when they are double. When the liquid found in these canals is aqueous, the animalculæ move about at freedom.

The capsules have a great affinity for water, which penetrates them by endosmose, and bursts them when they are simple. When there are two membranes, the outer one cracks, and allows a long prolongation from the internal membrane to project through it. The movements of the ova of the cephalopoda are not produced by muscular action, but in consequence of this endosmose.

The compound ova of many of the leeches, planariæ, and cephalopoda, are formed in the same manner, and exhibit still further the close resemblance between the ovule and the spermatic animalculæ.

In the testicles of the snake, small brilliant bodies are met with, which become elongated in form, when they reach the epidydimis, and in the *vas deferens* acquire a tail. Thus it appears that in these animals it is easy to trace the developement of the spermatic animalcule at its different periods of growth. These brilliant bodies are thus proved to be but the rudiments of the spermatic animalcule, around which first a head, and then a tail appears. They are to the spermatic animalcule what the vesicle of Purkinje is to the ovule.

MORBID ANATOMY.

Abnormal situation of the Right Kidney. By M. Lacauchie. *Bulletin de l'Academie Royale de Medecine,* Oct. 1840.) In examining the body of an officer of artillery, amongst other anomalies, M. Lacauchie found the right kidney situated over the brim of the pelvis, to the right of the sacro-vertebral articulation, partly in the pelvic, and partly in the lumbar region. It was covered with peritoneum and a pretty thick layer of fat. The kidney was less voluminous than usual, and its posterior surface accommodated itself to the osseous bed on which it lay. Two arteries entered it at its upper extremity; the largest artery was given off from the right common iliac; the other, which was longer, but not so large, arose from the origin of the left common iliac artery. The veins formed two unequal bundles; the larger came off from the upper portion of the kidney, and joined the right common iliac and left lumbar veins; the smaller bundle came off from the lower and back-part of the kidney, and joined the right internal iliac vein. Three calyces projected from the kidney on its anterior surface, and gave off three separate ducts which, after a short course, united to form one ureter, which maintained its usual course in reference to the *vas deferens* on its way to the bladder.

Case of Obliteration of the Vena Cava Inferior. By Dr Gely. (*Gazette Medicale de Paris,* 7th November 4840.)—A seaman, in the winter of 1838, was admitted into the Hospital at Nantes, with symptoms of anasarca, ascites, febrile oppression, extreme varicose dilation of the veins of the lower extremities, and of the abdomen, which were also excoriated over several spots. He stated that the varicose state of the veins came on during the winter of 1823, when he had the misfortune to have his legs frozen. He, however, recovered so far as to be able again to go to sea, where he contracted one of those cutaneous affections so common amongst negroes. Ulcers and abscesses formed on various parts of his legs, and he was long confined to bed; the varicose state of the vessels increased; and, for the last five or six years, he had been unfit for work. After a residence of a few weeks in the hospital, he died, with all the symptoms of some serious affection of the circulatory organs.

On dissection, the superficial veins of the lower extremities presented the appearance of thick cords, doubled on themselves a great number of times, in the same way, indeed, as the *vas deferens* is at its origin. This mass, as it approached the crural arch, increased in volume; but after passing this, in its progress over the abdomen, it diminished in bulk till it reached the false ribs. The varicose veins were perceptibly larger and more flexuous on the right than on the left side. The varicose vessels over the trunk of the body were the abdominal tegumentary vessels, which anastomosed on each side with a large external mammary branch, which terminated in the axilla. The femoral and iliac veins of the right side were filled with false membranes to such an extent as to diminish the calibre of the venous canal to a tenth of its natural diameter. The false membranes became more numerous as they approached the *vena cava,* which was reduced to the state of a cartilaginous cord, as far as the point where the emulgent veins unite with it, above which portion it was pervious, but reduced in diameter.

The right epigastric vein was somewhat narrowed in diameter, but that of the left side was enormously dilated, ascended towards the umbilicus, coursed along the suspensory ligament of the liver, and followed exactly the course of the umbilical vein. The two renal veins were very much dilated, and on the right side, an abnormal branch opened into the *vena cava*, and was continuous with the *vena comes* of the crural nerve, which was much dilated, and seemed to receive the blood from the deep vessels of the thigh.

The heart was hypertrophied, with dilation of all its cavities; and osseous concretions were met with around the aortic and auriculo-ventricular orifices, as well as in the coats of the aorta.

This case is extremely curious, as showing the mode in which the venous circulation had been kept up in spite of the destruction of the canal of the *vena cava*. Thus, on the right side, the blood from the lower extremity, collected by the superficial veins, passed along the tegumentary veins and external mammary into the axillary vein, and from that into the superior cava; whilst the blood of the deep portions of the same limb was emptied into the *vena comes* of the saphena nerve; and from it into the *vena cava* inferior, above the contracted portion, and lumbo-vertebral venous plexus. On the left side, again, the superficial blood returned to the centre of circulation in the same manner as that on the right side; whilst the venous blood from the deep portions of the limb was poured into the epigastric vein, and from it into the sinus of the *vena portæ* by the umbilical vein.

The opening of one of the epigastric veins into the umbilical vein, and the enormous dilation of the vein which accompanies the crural nerve, and communicated with the *vena cava* inferior, were the two most striking anomalies in this case.

Case of Cyanosis in an adult from the Foramen Ovale being open. By Dr Flechner of Vienna. (*Medizinische Jahrbuch des Osterreichischen Staates*, Vol. xxx. 1840.)—The subject of this observation was a man 54 years of age, who had been all his life engaged in a foundery of cobalt. He had never suffered a day's illness, but was observed to have a livid colour, with bluish lips, and slight dyspnœa. This last during the latter years of his life assumed the form of humid asthma, and terminated in dropsy of the chest.

On dissection, the pericardium was found very thin, the heart very large, with its right ventricle much dilated, and its walls increased in thickness. The *foramen ovale* was patent, but the *ductus arteriosus* was closed. No vestiges of tubercles were found in any part of the body.

The whole members of this man's family were asthmatic, and had a bluish colour; in some the colour was even livid.

MATERIA MEDICA.

Remarks on the Tinctura Opii Ammoniata of the Edinburgh Pharmacopœia. By J. H. Gilbert, Nottingham. Being a student in the laboratory of the Glasgow Royal Infirmary during the summer of 1839, it was occasionally my duty to dispense the *Tinctura Opii Ammoniata*,—a preparation, the apparently unchemical composition of which led me to suspect, that, as far as the opium employed is concerned, it could be of no utility; it being well known that ammonia acts as a precipitant to morphia. It is true that a great excess of that alkali will, in some solutions of the salts of morphia, either prevent its precipitation, or redissolve it when precipitated. In order, therefore, to ascertain the fact in the present instance, I submitted several separate portions of the tincture to examination at Dr Thomson's laboratory, where also I was a student.

Two ounces were first boiled in a flask with magnesia; the magnesian precipitate collected and boiled with alcohol; the alcoholic solution filtered while hot, and set aside in a warm situation to evaporate slowly: no crystals were deposited, and the residual matter, when tested, was found to contain no morphia.

Four ounces were next treated in a manner varying from the above. The liquid, previous to further treatment, was saturated with dilute acid; for when, as before stated, morphia has been held in solution by an excess of ammonia, I have found that, after expelling the greater part of that excess by gentle heat, and then saturating with dilute acid, a salt may be obtained; from the solution of which the morphia may be separated by the usual means. In the present case, however, no morphia was detected.

Having thus failed to detect that principle in the filtered tincture, the dregs of a known quantity were macerated for some time in water acidulated with muriatic acid;—the filtered liquid, after being digested with animal charcoal, was sufficiently concentrated on a water bath, and set aside, when a large quantity of crystals of muriate of morphia was deposited. A part of these was treated with ammonia, and the morphia tested in a separate state with a neutral solution of the perchloride of iron; and part, while in the state of muriate, was tested with iodic acid in the usual manner. As the dregs employed were not exhausted, nor the mother liquor treated with ammonia, an accurate quantitative result could not be obtained; but the amount of muriate of morphia was so great that it could not fall much short of the average produce of such a quantity of opium.

These experiments I had considered as conclusive. But, finding the new Edinburgh Pharmacopœia retained the old formula, and that the remarks made on the tincture by various writers on materia medica and pharmacy seemed to indicate previous experiment, I feared that either too small a quantity had been operated upon, or that some source of error had been overlooked. To obviate any such mistake, several portions, each of twelve ounces, were successively examined; in none, however, was morphia detected. As a check to the processes, several portions also of tincture of opium, (each of which would contain, by estimate, a quantity of opium equivalent to that in twelve ounces of the ammoniated tincture,) were treated in precisely the same modes as the ammoniated tincture had been. In these cases morphia was separated without difficulty; thus proving the efficacy of the processes.

It may be observed, that, before any of the tinctures were boiled with magnesia, the greater part of the ammonia and alcohol in the one, and of alcohol in the other, was expelled by gentle heat on a water-bath; as, in some cases, the alcohol in the liquid would be sufficient to take up the morphia after the decomposition of its salt by the magnesia. The loss of ammonia caused no deposition of morphia.

These facts seem to show that, if, as I believe is universally admitted, the medicinal effects of opium are attributable chiefly, if not entirely, to the morphia which it contains, the opium employed in this tincture might, with advantage, be omitted.

Nottingham, September 14, 1839.

MEDICAL PATHOLOGY AND THERAPEUTICS.

Case of Complete Paralysis of the Fifth Pair of Nerves. By M. C. James. (*Bulletin de l'Academie Royale de Medecine*, 30th November 1840.)—De Snoeck, a tailor, 36 years of age, in the month of March 1837, received a severe blow on the right side of the head, which deprived him of consciousness for a short time; but a few days sufficed to restore him to his wonted health. About the month of October 1838, when living in a cold damp apartment, his face was exposed to a current of cold air for a considerable time, after which he began to complain of flying pains over it. It was not, however, till the beginning of December, that they became very intense, and concentrated themselves in those parts which are supplied with the nerves of the fifth pair. They were confined to the whole of the right side of the forehead, temple, eye, nose, cheek, chin, lips, the right half of the tongue, the interior of the right ear; in fact, the whole of the right side of the countenance. During the crisis of the pains the muscles of the right side of the face were convulsively drawn outwards. To the paroxysm of pain succeeded a state of numbness in the affected part, which state was occasionally interrupted by acute lancinating pains. The patient never lost consciousness during these paroxysms; and no convulsive movements of the muscles of the trunk or extremities were remarked, the whole of the phenomena being confined to the right half of the countenance. When the pains somewhat moderated, the patient became aware that he had lost the power of seeing, hearing, smelling, or tasting, on the right side, as well as the tactile sensibility of the right half of the countenance.

Various remedies were at this time tried for his relief, but all in vain; and on the 21st of July 1840, he entered the Hospital of La Charité, under the care of M. Andral. He was found to present the following symptoms: Vision of right eye so completely lost, that he could with great difficulty distinguish light from darkness. The eye presented the same appearance as the sound one; but the sensation was completely gone; the conjunctiva could be pricked, or irritating bodies applied to it without the part giving any marks of their presence being felt. The sense of smell in the right nostril was also completely gone. The pituitary membrane could be pricked or irritated with ammonia, &c. without the patient being aware of the fact. The right half of the tongue has lost the sense of taste, as well as its sensibility—the median line forming the line of demarcation between the healthy and the diseased sides. Pricking the tongue, applying acids, salt, pepper, tobacco, colocynth, &c. were equally unfelt and unperceived by the patient. During mastication, when the morsel escaped to the diseased side of the mouth, its presence was not felt—the teeth, jaws, gums, and interior of the right side of the mouth participating in the loss of sensation. The right side of both lips from the median line had lost the power of sensation, so that when the patient drank out of a vessel, it seemed to him as if the bowl was broken off abruptly at the median line. The movements of all these parts were, however, free, and the tongue could be protruded in all directions. The hearing of the right ear was also lost; only a confused noise was heard when he was spoken to in a very loud voice. The sensibility of the internal conduit of the ear was lost; but the external concha retained its sensibility. From the median line in the middle of the face to the right ear, all sensation was lost over the right side of the countenance; and the tissues, deep as well as superficial, participated in this state. The movements, however, of these parts, and the general expression of the countenance, were unaltered; and it was impossible, from looking at the patient, to tell which side was the diseased one.

Electricity applied locally to the parts, particularly to the nerves where they come out at the supra and infra-orbitar and mental foramina, sufficed to effect a complete cure. Needles were introduced to conduct the electricity to the parts chiefly affected, and the shock passed through them. Thus when the eye was acted on, a needle was introduced through the upper eyelid, and another through the lower, and the electrical shock passed thus through the globe of the eye. What is a very curious circumstance, is, that the senses of seeing, hearing, smelling, and tasting, returned long before the common sensation was restored. About forty sittings of about twenty minutes each were necessary to effect a cure.

M. James remarks, that the above symptoms are precisely those which section of the fifth pair of nerves within the cranium, when effectually done, invariably produces, as he has often had occasion to observe in the experiments of M. Magendie.

Successful Treatment of Croup by repeated Emetics. By Dr JOURDAIN of Binch. (*Gazette Medicale de Paris,* 7th November 1840.)—Dr Jourdain praises the effects of tartar emetic in croup, and considers it as one of the most effectual modes of treatment yet known. He administers the antimony in half-grain doses, which, being dissolved in a few ounces of water, and given in three successive portions, he says rarely, if ever, fail to produce free vomiting and complete alleviation of the symptoms of croup. The dose is repeated, if necessary, after the lapse of every twelve hours, and he says, he has rarely found it necessary to repeat it oftener than three times.

This mode of treatment is not new, and, however well it may answer in mild cases, must be merely used as an auxiliary in those severe forms of the disease we are accustomed to meet with in this country.

SURGICAL PATHOLOGY AND THERAPEUTICS.

Removal of one of the bones of the Metacarpus, the corresponding finger being preserved. By M. BLANDIN. (*Bulletin de l'Academie Royale de Medecine.* Oct. 1840.) M. Blandin presented to the Academy of Medicine two patients from whom he had removed one of the bones of the metacarpus, but preserved the corresponding finger. The first case was that of a woman, the first metacarpal bone of whose right hand was affected with caries. The affected bone was cut out; and when the wound was healed and the cure completed, the thumb was found to be much shortened, but very useful, quite mobile, and sufficiently strong for most purposes. She was able to use the thumb for holding the pen or needle, and used it easily both in writing and sewing. The hand, indeed, was both less mutilated, and much more useful than if the entire thumb had been removed. This was the fifth time that M. Blandin had successfully performed this operation.

In the second case it was the fifth metacarpal bone of the right hand which was diseased; and though the operation for its removal was more severe than the first, when the cure was completed the finger was not less useful, and no mutilation of the hand was noticed.

A Hair extracted from the anterior chamber of the eye. By M. PAMARD. (*Bulletin de l'Academie Royale de Medecine,* Nov. 1840.) A man consulted M. Pamard for an affection of his eye, as he saw something like a hair opposite the clear portion of the cornea. On examination, a round body, from the centre of which projected a hair, was perceived in the anterior chamber of the eye. This body was extracted through the cornea, and found to consist of a piece of silex, enveloped in a mass of plastic lymph, and from the side of which projected a small hair. M. Pamard was unable satisfactorily to account for the appearance of the hair in the mass of lymph; but M. Velpeau thought it was nothing else than one of the eyelashes carried into the eye with the fragment of silex, and over which the cornea had cicatrized.

Case of Section of the right anterior Cord of the Spinal Marrow between the sixth and seventh cervical vertebræ. By M. BEGIN, (*Bulletin de l'Academie Royale de Medecine,* November 1840.)—M. Lafontaine, on the 22d of October, received a violent blow on the lateral and posterior part of his neck. He fell immediately on the right side of his body, and explained his fall, by supposing he had received a heavy blow from some blunt instrument. When he was examined a few moments afterwards, it was found that he had completely lost the power of motion of the right lower extremity, and partially that of the right superior extremity. The sensibility, however, of the whole body remained entire. He was carried to the Hospital of Val-de-Grace, where he was carefully examined by M. Begin, who found that, a few lines distant from the spinous process of the fifth cervical vertebra, there existed a straight transverse wound, the lips of which exactly applied to each other, and around which was neither inflammation nor marks of contusion. The limbs remained in the same paralytic state, but sensations of weight and formication were complained of in the arm. The pulse was full, but otherwise natural; and the skin was somewhat hotter than usual. The apparent trivial nature of the wound in the neck contrasting so remarkably with the severity of the symptoms, made M. Begin suspect that the anterior cord of the spinal marrow was injured, probably in the middle of the origins of the nerves of the brachial plexus, and below the origin of the diaphragmatic nerve. He was led to suspect that this was the situation of the injury from the respiratory movements being still entire, and the power of motion of the arm being partially retained.

The patient survived till the 27th, when delirium and convulsions came on; the pulse became irregular, and the respiration difficult, and he died at eight in the morning. No erection of the penis was ever observed.

In the cervical region the wound was united, and the cicatrix could scarcely be observed. When the skin was dissected off, no muscular union had taken place. Some blood had been effused at the point of their division.

When the sixth cervical vertebra was laid bare, the point of a knife was found fixed in and broken off on its right side. The instrument penetrated the spinal canal, where, in its course downwards and to the left side, it had cut through the right antero-lateral cord of the spinal marrow, from the level of the emergence of the posterior filaments, to the anterior median groove; it then traversed the whole thickness of the body of the seventh cervical vertebra, and its point had partially penetrated the posterior wall of the pharynx, where it was arrested.

On the Influence of Supra-orbital Wounds in the production of Amaurosis. By Dr Fr. De Walther. (*Journal der Chirurgie und Augenheilkunde*, October 1840.)—After a full and impartial review of the recorded cases of amaurosis, said to have been produced in consequence of wounds over the seat of the supra-orbital nerve, M. Walther arrives at the conclusion, that injury of this nerve has nothing to do with the production of this affection, since the nerve is often divided without giving rise to amaurosis, and amaurosis has been produced when the nerve is left quite entire and unaltered. M. Walther endeavours to prove that the supra-orbital nerve has no direct communication with the optic nerve or the retina, and that its connection with the ciliary nerves is very indirect, being by means of the nasal nerve. Amaurosis, which occurs after a wound over the supra-orbital region, he therefore attributes either to some direct violence having been sustained by the organs within the orbit, as by concussion, or to an affection of the brain; he, however, admits that amaurosis may also be produced in consequence of inflammatory action being propagated from the supra-orbital wound to the retina or optic nerve, through means of the continuity of the tissues.

Case of Dislocation of the Cervical Vertebræ cured. By Dr Schuh of Vienna. (*Medizinische Jahrbuch des Osterreichischen Staates*, Vol. xxx. 1840.)—A man, 24 years of age, whilst engaged at his work on the 5th of December 1838, twisted his head suddenly round, in consequence of one of his companions roaring into his ear, when he instantly felt something give way in his neck, and found it impossible to move his head. Next morning when he applied for assistance, his face was swollen, his head turned to the right, and bent down towards the shoulder. His neck was slightly arched on the left side, but hollowed out on the right. He complained of pain, which was augmented on pressure over the seat of the third, fourth, and fifth cervical vertebræ, and he was unable to move the head in any direction; every attempt to do so gave pain. The direction of the spinous processes of these vertebræ could not be accurately ascertained. He complained of weakness of the right arm, and could only raise it with great effort. The other functions of the body appeared to be natural. It was thus apparent that partial dislocation of some of the cervical vertebræ existed, and some attempts were made to reduce it by drawing the head directly upwards, the trunk of the body being held fixed; these, however, failed, but they gave no pain.

On the 7th of December the weakness and numbness of the right arm were greater. New efforts were therefore made to effect the reduction of the dislocation. The patient was laid in the horizontal posture, the shoulders were held fixed by means of folded sheets, whilst a towel was passed under the chin, in order to allow of a greater force being used for the extension; an assistant supported the occiput with both his hands. Extension was then made and gradually augmented, till the patient and assistant felt a snap as of two bones meeting. The extension was then gently relaxed, when it was found that the head was restored to its normal position, and the power of moving it was restored. The weakness of the limb, however, remained, and was even worse next day. On the 9th, he complained of vertigo and starting during his sleep, and his pulse was quick. For this he was bled to a considerable extent, which induced fainting and convulsions. He passed, however, a good night, and next day his pulse was nearly natural, the vertigo was gone, and he had partially regained the use of the right arm. He left the hospital cured on the 13th.

Violent Inflammation of the Urethra apparently caused by eating Frogs. By Dr Guyon. (*Gazette Medicale de Paris*, 13th February 1841.)—Many of the soldiers, and a considerable number of the officers who formed part of the army, sent into the province of Constantia, near Algiers, were last summer attacked suddenly with violent inflammation of the urethra, sometimes attended with even complete suppression of urine. The attendant puriform discharge was in very small quantity. The symptoms generally disappeared in a few days. This inflammation could not be traced to any venereal contagion, as the column to which these patients belonged had been at that time for more than a month at a distance from population. The physicians, the officers, and the soldiers themselves attributed the urethral inflammation to the eating of frogs, which occurred in great abundance in the places where these men were stationed, and of which all eat freely. This disease was most prevalent at Setif and Ain Babouche.

The frog to which this disease was attributed was recognized to be the *Rana esculenta*; and M. Larger remarked that, at the time the disease was most prevalent amongst the soldiers, species of Cantharis, Mylabris, and other neighbouring genera occurred in great abundance on the water plants in the marshes, where the frogs were caught, and that these insects at that time constituted no small part of the food of the frogs. M. Larger was from this inclined to believe that the poisonous properties of certain Coleoptera could pass unaltered through the bodies of these animals, without being affected by the process of nutrition, and act deleteriously on the human being who partook of their flesh.

M. Guyon endeavours to account for the occurrence of the disease from general causes, as, the extreme heat and stimulant food, which, he says, he has seen produce highly acrid urine, giving rise to pain in the bladder, and difficulty in passing water.

OBSTETRICS.

On the length and strength of the Umbilical Cord at the full term of Pregnancy. By Dr NEGRIER. (*Annales d'Hygiene Publique*, January 1841.) A girl of bad character was accused of having strangled her child by means of the umbilical cord, before it was completely expelled from the uterus. As there was a difference of opinion amongst the medical men as to the possibility of the umbilical cord possessing sufficient strength or length for this purpose, Dr Negrier performed a number of experiments, for the purpose of ascertaining the strength of the cord, and measured it in 166 cases to arrive at its average length.

Of the 166 cases it was remarked that in 144 the umbilical cord floated free within the uterus; in 20 cases it was rolled around the neck of the child; in one it was round the shoulders; and in one between the thighs, the breech presenting in this case. 98 of the umbilical cords were not varicose, and 68 were varicose. As to length, 28 were 17 inches long, 112 were from 17 to 25½ inches long, and 26 above that length.

The resistance of the umbilical cords was ascertained by attaching weights to one end of the cord until it ruptured, the weights being always attached to the placentar extremity. About one-half of the cords were passed by their middle over a round bar, and weights attached till they gave way; the other half of the number were rolled once and a-half round the same bar, covered with linen, so as to bring it to the diameter of a child's neck, when it was found that these supported a greater weight than those over the plain bar. The varicose umbilical cords were ruptured with a lesser weight than the sound cords, and generally gave way at one of the varicose dilatations. The mean weight which these varicose cords supported before they gave way was 8 pounds Troy; the most resistant supported 14 pounds 7 ounces. The medium resistance of the non-varicose umbilical cords was 14 pounds 4 ounces Troy; but one cord required 25 pounds 3 ounces to rupture it.

Dr Negrier next made a few experiments to ascertain what weights suspended round the neck of an adult would produce such a degree of compression as to cause unpleasant feelings or strangulation. A weight of 8 pounds was suspended to a cord passed once and a-half round the neck, the back of the neck being upwards. The respiration was rendered difficult, and the brain strongly congested in two minutes. Vertigo commenced soon afterwards. The respiration, however, could be continued with difficulty. When the face was placed upwards the effects of the congestion were more rapid; the respiration was much impeded, but was still possible; but Dr Negrier thought that death would have resulted if this position was maintained for a quarter of an hour.

When the experiment was made with a weight of 13 pounds, and the face downwards, rapid congestion of all the vessels of the head took place; the eyes became injected, and filled with tears; the respiration was very laborious, but was still possible. It was, however, dangerous to continue the experiment for two minutes.

When the same experiment was repeated, but with the face looking upwards, the strangulation was almost complete. Respiration was so impeded that Dr Negrier thinks death would have resulted in less than five minutes.

From these facts, he infers that the umbilical cord is both long enough and strong enough to produce strangulation in a new-born infant, by being twisted round its neck after the head is delivered. A force applied to a cord equal to 13 pounds would strangle an adult in five minutes, and a much lesser force would strangle a child.

On the best and surest mode of inducing Premature Labour. By Dr MEISSNER of Leipsic. (*Medizinische Annalen*, No. 4 of Vol. vi.)—Dr

Meissner from experience condemns all the usual means of inducing premature delivery, as none of them are free of danger to mother or child. Thus puncture of the membranes, by allowing the *liquor amnii* to escape, often causes death of the child; dilatation of the uterine orifice by means of prepared sponge is tedious, and apt to give rise to dangerous uterine irritation, as is also separation of the membranes, and titillation of the vaginal portion of the uterus; whilst ergot of rye, borax, &c. are often fatal to the child and dangerous to the mother.

During the last five years he has attended 900 deliveries, and has induced premature labour in eight with perfect safety to both mother and child. In all these cases the women were deformed or rickety, and their pelves were diminished in their antero-posterior diameter so much as in their previous deliveries to require perforation of the head of the infant in order to effect their delivery.

Dr Meissner never induced premature labour till the thirty-sixth week of pregnancy, and in all the cases the antero-posterior diameter varied from two inches and three quarters to three inches one line. His mode of operating, which is a modification of that by puncture of the membranes, was the following. A very slender canula is provided of about 13 or 14 inches in length, and bent regularly in the form of a segment of a circle. It has a ring soldered on the convex side of its lower extremity, in order to give a more secure hold, and allow of the point of the instrument being accurately guided. This canula is provided with two trochars, one with a blunt point and the other with a sharp-cutting point. When the instrument is to be used, the blunt-pointed trochar is introduced to the canula, and projects beyond its orifice so far as to prevent the edge of the tube injuring the parts. The patient then stands before the practitioner, who kneels before her, the usual manner in which vaginal examinations are made on the continent, or she may sit on the very edge of a chair or of the bed. The forefinger of the left hand is then introduced into the vagina, and the canula guided along it to the orifice of the uterus, making the convexity of the instrument correspond to the curve of the sacrum. The point of the instrument is then pushed slowly backwards and upwards, so as to make its rounded point slide between the uterus and the back of the membranes. When the point is once past the neck of the uterus, it advances easily, care only must be taken to detach the membranes as little as possible. When the point of the instrument is about 10 or 11 inches within the *os uteri*, the blunt-pointed trochar is withdrawn, and the handle of the instrument is pressed against the perinæum, to detect, if possible, against what the point of the canula is pressing. If it be felt to be a hard body, the point is made to move to one side or other till an elastic fluctuating spot be reached, which shows it is opposite the membranes alone; the sharp-pointed trochar is then introduced and perforation of the membranes is made. The trochar is then withdrawn, and about a table-spoonful of the *liquor amnii* is allowed to escape; after this the canula also is removed, and the woman is allowed to walk, sit, or lie down at her own pleasure. The amniotic waters after this slowly, and drop by drop escape, and appear to prepare the parts for delivery, so that after 24 hours the hand may easily be introduced into the vagina. Labour-pains usually come on from 24 to 48 hours after the puncture of the membranes; in several of the cases, delivery was completed 36 hours after the puncture.

Successful case of Cæsarean Section. By Dr KOENINGSFELD. (*Casper's Wochenschrift fur die Gesammte Heilkunde*, Oct. 1840.)—The woman who was the subject of this operation had the pelvis excessively deformed in consequence of rickets; the antero-posterior diameter being about two or two and a-half inches. The sacrum and the lower lumbar vertebræ were so deeply sunk forwards into the pelvis that the lower portion of the spinal column resembled a deep gutter bounded laterally by the posterior margins of the *ossa ilii*. The woman had been in labour for some days before Dr Koeningsfeld was called. He immediately performed the Cæsarean section in the *linea alba*, and extracted a live child. The operation was completed within a quarter of an hour. The woman made a good recovery, and within a month was quite well and able to nurse her child.

TOXICOLOGY.

Case of Poisoning by Tobacco. By M. TAVIGNOT. (*Gazette Medicale de Paris*, 28th November 1840.) A strong man, of 55 years of age, had a tobacco enema administered to him for the relief of ascarides in the rectum. The enema was ordered to be composed of one drachm and a-half (60 centigrammes) of tobacco leaves in about six ounces of water, but by mistake 15 drachms (60 grammes) of tobacco were used, and administered before the mistake was discovered. Seven or eight minutes had scarcely elapsed from the period of its administration before stupor, headach, and extreme paleness of the face made their appearance; pain was complained of in the abdomen, the speech became thick and indistinct, and slight convulsive movements were observed. A purgative enema was immediately administered, stimuli and strong coffee were also given, cloths dipped in cold water were applied to the head, and sinapisms to various parts of the body. He was also bled pretty freely.

Notwithstanding all these means, the paleness of the face and surface of the body increased, the expression of the face was indicative of pain and stupor, the pupils of the eyes were natural, the respiration became more and more laborious and slow; his intellectual faculties seemed to be greatly weakened, but he still occasionally understood questions which were put to him, though unable to answer to them distinctly; convulsive tremors were first observed in the arms, but soon extended to the legs and trunk of the body, and went on increasing in severity for six or seven minutes, after which a state of complete prostration came on, attended with slow very painful respiration. Well marked coma, with complete relaxation of all the muscles of the body, preceded the fatal termination, which took place about eighteen minutes after the administration of the enema. The pulse stood at 68 before the blood-letting, and 44 after it. No vomiting occurred in this case.

Case of Poisoning with the Acetate of Lead, in which the poison was detected in the Urine. By MM. ORFILA and VILLENEUVE. (*Bulletin de l'Academie Royale de Medecine*, Jan. 1841.)—A girl, in a fit of despair, swallowed between eight and ten drachms of the superacetate of lead. She was speedily affected with prostration of strength, paleness and coldness of the surface of the body and faintings, which symptoms were in a short time succeeded by vomiting and precordial anxiety. Sulphate of soda was given in large and repeated doses, and was followed by copious alvine evacuations. Under this treatment the poisonous symptoms went off, and the temperature of the body returned. The urine which the girl passed twenty-five hours after swallowing the poisonous dose was examined by M. Orfila, who extracted from it a considerable quantity of lead, showing that the poison which had been absorbed was thrown off from the system by means of the kidneys.

M. Lassaigne of Alfort made a number of experiments at the veterinary school there, with the view of ascertaining in what secretions or organs the lead with which animals has been poisoned is found. He constantly met with it in large quantity in the venous blood and in the urine of living animals, and in the liver and kidneys after death.

MM. Chevallier and Brichateau have examined the urine of the workmen in the lead manufactories, especially at the time when they were affected by this metal, but have as yet, been unable to detect it even in the most minute quantity in that fluid. M. Orfila, also, was unable to detect its presence in the body of a child supposed to have been poisoned from breathing its vapours.

On the best mode of Treating Cases of Poisoning by Arsenic. By M. ORFILA. (*Bulletin de l'Academie Royale de Medecine*, Nov. 15, 1840.)—Rasori and Giacomini sometime ago advanced, that the ordinary antiphlogistic treatment usually practised in cases of poisoning by arsenic was decidedly hurtful, and that, from their experiments on 47 dogs, a stimulant and tonic plan of treatment was that best fitted to effect a cure. With the view of ascertaining the truth of this statement, M. Orfila, in the presence of a commission of the Royal Academy of Medicine, and of many of his friends, experimented on 157 dogs; and the following is a short abstract of his extended inquiry:

1. Twelve dogs had their œsophagus tied, and the ligature left on for thirty hours. When removed at the end of this period, the animals eat and drank freely, and seemed no ways injured; and the wound in the neck healed in a few days. This experiment was for the purpose of ascertaining what amount of suffering was to be attributed to the ligature in the subsequent experiments, where the œsophagus was tied to prevent the poison being vomited.

2. To three dogs were administered the stimulant and tonic mixtures, said by the Italian physicians to be so successful in the treatment of arsenical poisoning; but they were found in every case to produce death in a few hours, the animals exhibiting all the symptoms of intoxication, and also occasionally acute internal pains. The mixture was composed of eight ounces of beef-tea, two ounces of brandy, and the same quantity of wine.

3. Arsenic in powder was introduced into the subcutaneous cellular tissue of the inside of the thighs of thirty-four dogs, and the following was the result:—Two grains was the quantity used for each dog; and the first five being left without any treatment died in from thirty to forty hours. Ten dogs were treated by the Italian tonic plan, and all died. Four were allowed lukewarm water, but also died, having passed very little urine. Seven dogs were bled about six or seven hours after the introduction of the poison; but they all died. Six dogs had diuretic medicines administered to them, composed of six pounds of water, one pound of white wine, and one ounce of saltpetre, with the occasional addition of a little Seltzer water, and all recovered. They passed large quantities of urine, which contained arsenic, as was ascertained by analysing it in the apparatus of Marsh. Four other dogs, treated by the saltpetre water died; but they scarcely passed any urine.

4. Forty-one dogs were made to swallow arsenic in powder, in quantities varying from three and a-half to ten and a-half grains, with the following results:—Four dogs whose œsophagus was not tied recovered without any treatment, having expelled the poison by free vomiting. Four dogs whose œsophagus was tied died in a longer or shorter time. Four dogs who were allowed to vomit recovered, although subjected to the stimulant treatment. A ligature was kept round the œsophagus of seven dogs for a variable length of time, from two to thirty hours, and the animals were subjected to the Italian stimulant treatment; four only died. Of those which recovered, two vomited after the removal of the ligature, which in one was after two hours and three-quarters, and in the others after five hours and a-half, but all passed an abundant quantity of urine. Four dogs had quinine administered to them in a decoction of cinchona bark; but their œsophagus was tied, and they all died. No urine, or only very little, was passed by them. Nine dogs who were subjected to the aqueous treatment, and were allowed to vomit, all recovered. They all vo-

mited and passed urine freely. Of nine dogs who were bled, seven recovered. In four of them the œsophagus was tied for three, four, and five hours, and blood-letting was the only remedy used. The other three were allowed to vomit.

5. In this series of experiments, the arsenic was dissolved in water, and then introduced into the stomach, the same quantities being used as in the former series of experiments. Seven dogs, three of whom had the œsophagus tied for three hours, died in a few hours, though several of them vomited. Eighteen dogs, who had each about four grains of arsenic administered to them, and had the œsophagus tied for from forty minutes to two hours, all died in spite of the Italian stimulant treatment. One large dog vomited a part of the solution, and recovered; it had also been subjected to the stimulant treatment. Eight dogs, which vomited freely a few minutes after taking the poison, and were subjected to the aqueous treatment, recovered. They all passed large quantities of urine. Another dog, which did not vomit for one hour, died, though it was similarly treated. Two others, which had the œsophagus tied, the one for three-quarters of an hour, the other for fifty minutes, died. Thirteen dogs were bled, but only two recovered. Nine dogs were bled, and had also hot water given to them, and seven recovered. The two which died had the œsophagus tied, and vomiting, of course, prevented for fifty minutes.

6. This series of experiments was undertaken with the view of ascertaining whether the exciting a copious diuresis would have the effect of obviating a fatal result, when other poisonous agents were administered. Tartrate of antimony was the first poison selected: and M. Orfila found, that one grain and a-half introduced into the cellular substance of the thigh produced death in four dogs in the space of from seventeen to thirty-six hours. Five dogs were similarly poisoned, but had diuretic drinks administered to them, and four recovered. They passed large quantities of urine, which was ascertained to contain antimony. The dog which died had passed no urine.

7. Opium was the poison next experimented on; and from fifteen to ninety grains of its watery extract were administered to twelve dogs, in some of the cases being applied to the cellular tissue of the thigh, at other times introduced into the stomach. But these dogs, though subjected to the diuretic treatment, passed no urine, so that he was unable to ascertain whether this poison, like the metallic salts, could be removed from the system by the action of the kidneys of these animals. He has, however, been able to ascertain that opium is passed off with the urine, having several times detected morphia in that fluid; and he thinks, however, that in man a diuretic plan of treatment might with propriety be adopted, as one means of freeing the system from the poison, diuresis being more easily induced in him, when labouring under the effects of opium, than it is in the dog.

The conclusions which M. Orfila draws from the above experiments are so very obvious, that it is unnecessary to detail them here. In poisoning with arsenic or other metallic salts, after free vomiting, the great object is to aid the expulsion of the poison from the system by exciting full and free diuresis. The Italian stimulant plan appears to be worse than useless.

MEDICAL POLICE AND MISCELLANEOUS.

Resolutions of College of Physicians of Edinburgh on Messrs Hawes' and Warburton's Bills for Medical Reform.—Edinburgh, 12th February 1841.—At a meeting of the Royal College of Physicians of Edinburgh, called to consider the report of a committee of their body, on the bills for medical reform, which have been introduced into the House of Commons by Mr Warburton and Mr Hawes, the following resolutions were unanimously adopted:—*1st.* That the college derive much pleasure from finding that the subject of medical reform, which has so often been under their consideration, and in which they have repeatedly endeavoured to interest the Legislature, has at last been brought, in a tangible form, under the notice of the House of Commons; and trust, that the full discussion which the subject has received, and is receiving from the profession at large, and the attention now about to be bestowed on it by Parliament, will lead to the removal of some of the evils of which the college have frequently had occasion to complain.

2d. That, in the opinion of this College, the great evil arising from the want of an uniform system of medical legislation throughout the united kingdom, is the possession, by particular corporations, of local privileges, which render their licentiates alone legally capable of acting as general practitioners in particular districts and portions of the country, to the exclusion of persons of equal, and it may even be, of superior qualifications.

3d. That, so far as the College is aware, the only plausible objection which has been urged against the abolition of these local privileges, and against the adoption of a system for placing the licentiates of all the medical corporations on an equal footing in respect of the right of practice, is the inequality alleged to exist, or actually existing, in the amount of medical education required of candidates for their licences by the several boards, and in the degree of strictness with which the examinations of such candidates are conducted.

4th. That whilst, therefore, with a view to the interest both of the public and the profession, community of privilege should, in the opinion of this college, be the primary object of any legislative enactment relative to the medical profession, sufficient education and examination must, at the same time, be duly provided for.

5th. That from the communications which have taken place, and the understanding that has been come to, between a number of the different boards with which the superintendence of medical education at present rests, little difficulty can now exist in fixing a minimum course of study, general and professional, without evidence of having passed through which, no one should be allowed to present himself as a candidate for a medical license. On this point the college will only further refer to the joint resolutions agreed on by the Medical Faculty of the University, and by the Royal Colleges of Physicians and Surgeons, of Edinburgh, of date October 1838.

6th. That to produce some approach to uniformity in the system on which the examinations of candidates for licenses are conducted by the several boards, and to secure the public against the admission of incompetent persons into the medical profession, it appears to this college that it would be desirable that some superintending body should be constituted, having authority to take cognizance of the manner in which the duty of examination is executed.

7th. That the persons of whom this board should consist might probably be most advantageously selected by the Crown from lists furnished by this and the other boards at present entrusted with the government of the medical profession.

8th. That in the opinion of this college, no measure of medical reform will be satisfactory which does not confer on a person who has once received a certificate of his fitness to exercise the medical profession from any of the established boards, the right of practising in any district of the country, or in any particular department of the profession, without the necessity of submitting himself to a second examination before another board.

9th. That whilst the college readily acknowledge that the proposal of creating a representative body or bodies, elected periodically by the profession at large, by which the affairs of the medical profession might be superintended and directed—(a proposal which forms so prominent a feature in the two measures which have been submitted to Parliament)—is desired by many most respectable members of the profession, they are disposed to believe that this desire has in a great measure originated in accidental and removable causes; and they are satisfied that any attempt to carry it into effect would be attended with serious inconveniences, if not insuperable difficulties.

10th. That, in particular, the college conceive that this proposal has in a great measure grown out of the dissatisfaction very generally prevailing among the members of the medical profession, not only with the local privileges of practice attached to the licentiates of particular corporations, as already noticed, but also with the narrow and exclusive system on which admission into the governing body of some of the medical incorporations has hitherto been regulated; and that the adoption by, or enforcement on, these corporations of conditions of admission of a more reasonable and liberal character, in obtaining for them the confidence of their licentiates, would in a great measure supersede the desire at present existing for a representative system of superintendence.

11th. That, among the obvious inconveniences with which the election of a representative body, by the medical profession at large, would be attended, the college conceive that it would have the effect of producing and continually renewing agitation and dissension among the members of the profession, and of directing their attention from far more important duties; whilst those who are best qualified for performing the duties that should be committed to a superintending body, would be least likely and least able to take those steps which are essential to the gaining of popular suffrages. And, as a further objection to the boards proposed, in the bills before Parliament, to be established for the regulation of the profession, it may be remarked, that, from the multifarious duties intended to be committed to their members, and the necessity which would be imposed upon them from time to time, of leaving their homes for execution of these duties, it would be impossible to obtain the services of persons of eminence and station in the profession; and that the appointments would therefore fall into the hands of an inferior grade of individuals, in whom neither the public nor the profession would have confidence.

12th. That whilst the college readily acknowledge that the actual constitution of some of the medical corporations in the united kingdom requires to be rendered more conformable to the spirit of the age, they are not disposed to admit that these institutions are so useless, or so incapable of amendment, as to make it advisable either to abolish them by direct, or to supersede them by indirect legislation, the acknowledged fact being that, notwithstanding any defects under which they may labour, the country is, through their agency, provided, at the present time, far beyond all precedent, with well-educated and judicious practitioners.

13th. That it would be desirable that the duty of examination should be remunerated in such a manner as to remove the possibility of a suspicion, that the examiners have a pecuniary interest in the number on whom they confer the license to practise.

14th. That, considering the large amount of services gratuitously rendered to the public by the medical profession, the very inadequate compensation received by a large proportion of its members for the long and expensive course of education necessary to qualify them to practise, and for the performance of its very laborious duties; and considering also the direct interest which the public has in being able easily to distinguish between qualified and unqualified practitioners of the healing art, the college are decidedly of opinion, that any additional expenses which might be occasioned by an improved system of medical legislation should be defrayed out of the public purse, and not by an annual tax upon the profession, as seems to be contemplated in the measures which have been submitted to Parliament.

15th. That the college consider that it would be highly desirable that a scheme should be arranged for the registration of licensed practitioners. That, in their opinion, none but registered practitioners should be legally eligible for any public professional situation; that the assumption of a professional designation by a person not entitled to it, should be declared an offence punishable at common law; and that the right of suing in courts of law for professional remuneration should be secured to licensed, and denied to unlicensed practitioners. But, in recommending enactments to these effects, for the encouragement of the licensed, and the discouragement of the unlicensed, the college is convinced that the suppression of unlicensed practitioners is beyond the reach of legislative interference, so long as their exists a taste for this species of practice in the public mind.

Gloucestershire Medical Association.—At a Meeting of the above Association held at the Gloucester Infirmary, on Tuesday, Feb. 16th, 1841, the following resolutions were unanimously adopted:—I. That this association, fully impressed with the importance of obtaining for the medical profession an improved organization, is nevertheless unprepared to support any of the medical reform bills which have yet been submitted for the consideration of the profession.

II. That the object of legislative interference with the profession, as far as medical polity and education are concerned, should be to insure a supply of well-informed and "honourable practitioners, who, while they fulfil their duties to society in the best possible manner, maintain for themselves a respectable station in it."

III. That any measure which proposes to effect this object, *by destroying the influence and authority of the existing medical colleges and corporations of Great Britain and Ireland, will assuredly and deservedly be rejected by Parliament;* and even could it by any violent proceedings of a portion of the faculty, be forced upon the country, it would fail of its professed design, and entail upon the profession a permanent loss of respectability.

IV. That the present distinctions and classes of the medical profession have originated in the necessities and requirements of the community, and could not be abrogated or amalgamated without a compulsory and arbitrary interference with the habits and feelings of society, and serious detriment to the cultivation of medical science.

V. This association is, however, of opinion, that every effort should be made to procure such a general representation of the members of existing colleges, in their respective councils and governing bodies, as may increase their stability, raise them in public estimation, and render them, what they are not under the present constitution, bulwarks of protection to the honour, security, and legitimate interests of their members.

VI. That such general representation would tend to secure a responsible application of corporate funds—would be the most effectual method of removing the prevalent and deeply rooted dissatisfaction of the majority of the profession, and would create a sense of mutual dependence, confidence, and unanimity, among its various members.

VII. That no proposed alteration of the existing mode of electing the governing bodies of these institutions deserves support, which would not confer an equal voice and influence in the general representation upon their respective members residing in *all* parts of the kingdom, and that any attempt, direct or indirect, to confine such influence to, or confer it unduly upon metropolitan practitioners, (whether those now in power or those who may be seeking for power) should be zealously opposed.

VIII. That the appointment of any persons, who are not members of the medical profession, to a share in the control and direction of its internal affairs, would not only be derogatory to its character, and prejudical to its welfare, but also unnecessary for the protection of the public, so long, at least, as the sanction of the ministers of the crown or the judges be required for the validity of any bye-laws made by the medical boards or councils.

IX. With every desire, therefore, to uphold and strength the established medical institutions, it must nevertheless be admitted,—

That the present mode of qualifying and licensing general practitioners in England is liable to serious objections; inasmuch as a large proportion of that class practise, either with the mere license of Apothecaries' Company, or without it as members of the College of Surgeons or graduates of the London University, (the company being unable to prevent such graduates and members supplying medicines from druggist' shops;) or, lastly, as licentiates of the College of Physicians, who have the right to practise both surgery and pharmacy.

That the existence of these several avenues to general practice produces an injurious effect, both on professional interests and on the progress of medical science, since each of the above-mentioned bodies is ready to confer its license, *independently of the rest;* each, likewise, derives pecuniary advantage in proportion to the number of diplomas it may grant, and consequently has a strong inducement to attract candidates, by lowering its standard of qualification and diminishing the severity of its examination; whilst, as might be expected under such a system, all of them exhibit remarkable diversity in the nature and extent of their requirements.

That it is, therefore, desirable to abolish so mischievous a rivalry, and to effect harmony and co-operation between these institutions, by combining their efforts in the important duty of providing the bulk of the community with medical attendance.

X. That this association further admits the importance of instituting an uniform minimum qualification for medical practice, and of requiring that every future candidate for admission into the profession should pass an examination in all departments of the healing art.

Also, that the office of conducting such an examination, and the power of conferring the primary qualification, should not be wholly committed to any existing college or corporation; both because the others would naturally object to the transfer of their present powers to a rival, and chiefly because *none singly is competent to test the fitness of the candidate for all branches of practice.*

XI. In order, therefore, to attain the desired object, without departing from any principle laid down in the foregoing resolutions, it is suggested—first, that the sole power of conferring this primary grade, and license to practice, after a comprehensive examination, might be entrusted to a board (in each metropolis) composed *entirely* of delegates from the several examining bodies which now grant medical licenses and degrees; secondly, that existing interests might be protected, all the legitimate objects of the present colleges be promoted, and the honour and welfare of the profession in general be maintained, by means of the funds arising from admission to practice by the metropolitan boards; and thirdly, that the licenses thus obtained should by no means be allowed to supersede or interfere with the regulations which any college of physicians or surgeons, or any university, might frame for the grant of its diploma and membership to those who, after taking the primary grade, may prefer to qualify themselves by a prolonged period of study, and by extended opportunities of acquiring knowledge for practice in a higher or an exclusive department of their profession.

XII. That the singular deficiency of any adequate provision for regulating the practice of pharmacy and the sale of medicines in this country, (in which respect we are an exception to the nations of Europe,) demands the speedy attention of the Legislature, and renders it important that the London Apothecaries' Company, besides contributing its share to the formation of the proposed licensing board for medical practitioners in England and Wales, should also be entrusted with full powers to examine and control chemists and druggists, as well as to superintend and inspect the importation, preparation, and sale, of drugs and medical compounds; thus affording to the public due protection, (which at present is entirely wanting,) against ignorant and unqualified pharmacists, and against a supply of inferior and adulterated medicines. Lastly, that any attempt on the part of the Apothecaries' Company to escape from the performance of these its legitimate functions, and to depart still further from the original purpose of its institution, should most strongly be deprecated, and most vigorously opposed.

At the same meeting, Mr Rumsey read a copy of the clauses intended to be proposed by Mr Sergeant Talfourd and Mr Wakley, to be inserted in the Poor Law Continuance Bill; these clauses being approved, a committee was appointed to draw up a petition to parliament, founded on the matters therein contained.

Petition of the Gloucestershire Medical Association.—That the bill now under the consideration of Parliament, for the continuance of the Poor Law Commission, and for the further amendment of the laws relating to the relief of the poor, affords an opportunity for improving the present imperfect and unsatisfactory provision of medical relief.

That in the year 1838, a committee of your honourable House examined several medical practitioners, who were unanimous in suggesting certain amendments of the present system, several of which were approved of and recommended by that committee, and have since been agreed to, although not adopted by the Poor Law Commissioners.

Your petitioners therefore humbly pray that provisions to secure the general adoption of these measures may be introduced into the said bill; and, in particular,

First,—That a medical director may be appointed to superintend all matters relating to the medical relief of the poor, to which department his whole time and attention may be devoted.

Secondly,—That the disgraceful and injurious system of appointing union medical officers by tender may be abolished; and that a rate of remuneration, calculated to secure an adequate supply of medicines and medical attendance to the sick poor, may be insured to those officers by the enactment of maximum and minimum limits to such remuneration, thus protecting, on the one hand, the rate-payers; and, on the other, the medical profession; thus also allowing the guardians of each union to determine the precise amount of payment within the prescribed limits, according to the custom and peculiar circumstances of the respective localities, and subject to the final decision of the medical director in case of dispute.

Thirdly,—That certain limits, to the extent and population of medical districts, may be defined.

And Fourthly,—That no future practitioner may be eligible to the appointment of medical officer unless he shall prove his competency to practice by passing examinations in medicine, surgery, and midwifery.

By the enactment of the above provisions, your honourable house would humanely confer a most essential benefit on more than a million poor, annually suffering from sickness, combined with destitution,—would afford important protection to the public in general, and would grant encouragement to the members of a useful profession in the performance of the most laborious and arduous department of their duties.

And your Petitioners will ever pray.

Military Surgery.—(From the Naval and Military Gazette, No. 421.)—The following extract, which concerns our Medical School, is abridged from the Naval and Military Gazette, and we are glad to observe from it that the possibility, which we hinted at on a former occasion, of the chair of Military Surgery in this University becoming the parent of similar institutions elsewhere, is likely to be realized. We know that the sentiments expressed in the following quotation are quite in unison with those entertained by the Professor of Military Surgery in this University, who has omitted no opportunity, either public or private, of advocating the establishment of similar Professorships in the other Metropolitan schools, and this, we believe, from a conviction of their importance to the national interests.

In the present improved state of Military and Naval Medicine and Surgery we see an ample field for such Lectureships without encroaching on any ground hitherto preoccupied; and in the conduct of them it is not exclusively to the teaching of surgery (as the name is sometimes thought to imply) that the attention of the professor ought to be directed. It must never be forgotten that in this University there have always been two very different descriptions of auditors in attendance on the class of Military Surgery; one to be instructed in surgery, which they have in some measure forgotten—the other, to be instructed in military hygiene, and the diseases of troops on foreign stations, which they have never learned.

" The attention which has of late years been paid to Medical Statistics, particularly those of the Army, so repeatedly noticed in our columns, seems at length to have brought that important subject under the consideration of our Transatlantic brethren; for among the documents accompanying the last Message of the President of the United States to Congress, we find a very interesting report by the Surgeon-General of the Army, on the sickness, mortality, and fatal diseases among the regular troops during the past year, from which we propose extracting a few facts likely to interest our military readers.

" By this document it appears that the total strength of the American regular Army for that year was 8,950; among whom the cases which came under treatment were 22,248; and the deaths, 214.

" This is a much higher proportion than among our troops serving on the American continent, as shown by Major Tulloch's statistical reports. * * * " At a time when every day is rendering more apparent the advantage of enabling persons destined for the medical departments of the army and navy, to make themselves acquainted, by documents such as these, with the diseases they are likely to encounter in every quarter of the world, and when so much remains to be done to ameliorate the condition and improve the health of the soldier, it must appear a matter of surprise and regret to all interested in the welfare of the army, that the important study of Military Surgery, under which such topics are embraced, should be entirely confined to the University of Edinburgh. Even in time of peace, the number of individuals who pass through our Military and Naval Hospitals at home and abroad, amounts to nearly one million annually, and includes the natives of almost every climate under heaven. In the East Indies the British medical officers of the regular, the irregular, and native armies, have at least 300,000 soldiers under their charge, besides some hundred thousand followers attached to them. The Turkish army has recently been placed under similar superintendence. In addition to the troops of the line, our colonial corps include thousands of Maltese, Negroes, Hottentots, Malays, and Cingalese, all liable to suffer under diseases in some measure peculiar to their race, and all requiring a very different mode of treatment to Europeans; besides, even the diseases to which the latter are subject, assume very different forms, and require very different modes of treatment in various climates. How then is all this knowledge, so essential to the due discharge of the duties of a medical officer, to be acquired, if the student graduates at any other University than that of Edinburgh? He must either be put to the inconvenience of pursuing his studies in another kingdom, or remain ignorant of this most important branch of his profession, till he acquires it by experience, at the cost perhaps of hundreds of the lives of his fellow soldiers.

" No medical candidate should ever be permitted to enter the army or navy, who has not attended at least one course of lectures on Military Surgery, embracing all those circumstances by which the health of soldiers and sailors is likely to be affected in different climates; and, to enable him to do so, it is indispensable that two Medical Officers of acknowledged talent in that department of their profession, should be nominated to chairs of Military Surgery in the Universities of London and Dublin, and whose duty it would be, to give to their pupils the benefit of the experience they have acquired in a long course of service, as well as of the vast mass of information annually transmitted from all foreign stations to the Medical Departments of the Army and Navy, and which we feel confident would most cheerfully be laid open to their inspection for that purpose. The trifling addition of L.100 or L.200 per annum to the half-pay of these officers, would probably be a sufficient remuneration on the part of Government; and we have no doubt, for that sum combined with the usual fees from their pupils, men of talent would be found who would deem it not less an honour than a pleasure, to employ their leisure hours in a manner so creditable to themselves, and likely to be attended with such benefit to their profession.

" That the study of such topics is likely to be a favourite one with the aspirants to medical honours, is sufficiently proved by the rapid increase in the Military Surgery class of Edinburgh, under its present Professor, Sir George Ballingall; for, independently of numerous Military and Naval officers who have availed themselves of the opportunity of attending his lectures, the annual average number of pupils *paying fees*, for the first five years was about 6 or 7 ; during the subsequent period, embracing thirteen years, the annual average has been nearly 50.

" Much of this success is no doubt attributable to the talent and urbanity of the Professor, but much also we conceive to be due to the interest which is taken in the subject, even by those who have no intention of devoting themselves to the military or naval departments of the profession. Now that this feeling seems daily on the increase, and is extending to foreign countries as well as our own, we deem it a favourable opportunity for impressing on our Legislature and the public, the necessity for supplying in the Universities of each of our capitals, the means of instruction in a science so essential to the health both of soldiers and sailors, and on which, in a great measure, depends the efficiency of the Army and Navy, and the successful operations of the state."

Harveian Society.—Notice to Students of Medicine.—The Harveian Society of Edinburgh have fixed on the following subject for their prize essay for the year 1842, viz.:—" An experimental inquiry into the Physiological and therapeutical properties of bromine and its compounds."

Dissertations on this subject must be transmitted to one of the secretaries, on or before the 1st day of January 1842. Each Dissertation must be accompanied by a sealed letter, containing the name and address of the author, and inscribed on the back with a motto. The same motto must also be prefixed to the Dissertation to which the letter belongs. None of the sealed letters are ever opened, except that bearing the same motto with the successful essay.

The prize given by the society, to the successful candidate, is an elegant silver medal, with a suitable inscription.

The candidate is at liberty to employ his essay afterwards, in any way he may think proper: and accordingly, some Dissertations submitted to the Harveian Society, have, in consequence of Publication, conferred no small reputation on the authors.

No candidate, who has already gained the society's prize, is allowed to compete a second time.

By order of the society,
Richard Huie, M. D., 8, George Square, } *Secretaries.*
P. D. Handyside, M. D., 24, Duke Street, }

INDEX.

Acidity, the cause of the menstrual secretion not coagulating 250
Ague, on the use of piperine in the treatment of, 98
Agues on those of Canada 348
Ahmedabad, on epidemic fever in 194
Alexander, *Prof.* See small-pox. 255
Amaurosis not produced by supra-orbital wounds 554
Amputation of shoulder-joint, with removal of scapula, successful 265
Anatomy, various new works on 244
Aneurism, aortic, opening into vena cava, case of 63
Animalculæ, spermatic, on their origin and development 547
Antimonial vapours, on poisoning by 265
Anus, fissures of, treated by rhatany root 263
Aorta. See Aneurism 63
—— and pulmonary artery, irregular disposition of 250
Arteries, carotid, on the effects of tying them 543
——, on their properties and influence on circulation 17
Artery, carotid, case of fatal rupture of from gangrene 125
—— pulmonary. See Aorta 450
—— —— case of absence of 251
Ascites, case of, cured by bandaging 260
Asphyxia, Dr John Reid on the mode of death in 437
Asylum of Bloomingdale, statistics of 269
Auscultation, MM. Barth and Roger, treatise by 486
Barry, *Dr Martin*, on embryology 474
Barth and *Roger*. See Auscultation 486
Bandaging, a remedy for ascites 260
Bell, *Mr B.*, cases of cerebral disease 458
Bile retained, a cause of jaundice 285
—— fluid like it contained in thyroid gland 454
Bigger M., case of want of pulmonary artery 251
Births, deaths, and marriages, first report on 156
Black, Dr Francis, on sanguineous tumours of the scalp 112
Blood. See Arteries and Holland 17
—— on variations in the proportion of its principles 544
Blood-letting and opium, on its use in inflammation 257
Blood-vessels of uterus and placenta, on their connection 1
—— —— on their contractility 545
Bombay, Medical and Physical Transactions 194
Bowring, *Dr*, on the contagion and non-contagion of plague 491
Boyd, *Dr*, his report on St Mary-le-bone Infirmary 304
Brain and digestive organs, treatise on the sympathy between them 526
Braithwaite, *Mr*, his retrospect of practical medicine and surgery 542
Bromilow, *Mr*, his account of a bicephalous monster 435
Breschet, on rabies, experiments by 256
Cæsarean section, case of, successful 557
—— —— two cases of, terminating fatally 67
Cancer, developement of, in the veins 251
Carotid arteries, two cases of rupture of, from sphacelus 125
Carotid arteries, on the effects of ligature of 543
Carson, *Dr*, on his views on the circulation 44
Cephalaematoma, on 112
Circulation of the blood, on the influence of the arteries on 17
Children, four at one birth, case of 268
Cholesterine substance found in a biliary abscess of the thyroid gland 454
Chord, umbilical, on its length and strength 556
College of Physicians, Edinburgh, their resolutions on the proposed bills for Medical Reform 568
College of Surgeons, Edinburgh, their Report on the proposed bills for Medical Reform 270
Congenital Hernia of Heart and Stomach 251
Contagion of plague, Dr Bowring and Mr Holroyd on 491
Corpora lutea, Dr Paterson on 395
Croup, use of emetics in 552
Croton oil, on its therapeutic action in nervous disorders 100
Death, sudden case of, from rupture of spermatic vein 253
Deformity, articular case of, cured by division of muscles, ligaments, and tendons 264
Doane, *Dr*, his translation of Meckel's manual of general Anatomy 244
Dunn, *Mr*, his vital statistics of Scarborough 487
Ear, on its mechanical functions, Dr Sym, on 48
Elliot, *Mr J.*, his report on the 65th 36th, 69th, 37th, and 8th regiments 335
Elliot, *Mr T.*, on strabismus 370
Embryology, Dr Barry's researches on, 474
Emetics, use of, in croup 553
Empyema, case of, successfully treated by operation 262
—— —— treated by operation, unsuccessful 361
Epilepsy and apoplexy, case of, 368
Exanthemata and enanthemata, on 255

Excision of neck of uterus, case of, followed by pregnancy . 104
Excision of one of the Metacarpal bones . . 553
Expectoration of milk at parturition 268
Fever, lake, of Canada, (ague and remittent) . . 388
Fever of Kutch, account of, . 194
Fibrine, on its existence in the blood in the shape of globules . 545
Fifth pair, complete paralysis of, cured by electricity . 551
Fissures of anus, treated by rhatany root . . 263
Foramen ovale open, case of, 550
Frogs, eating, succeeded by urethral inflammation . . 555
Funis umbilicalis, on its length and strength . . 556
Ganglionic nerves, functions of . 249
Gilbert, Mr J. H. on the ammoniated tincture of opium . 550
Gilder, Mr T. his Report on the Fever of Ahmedabad . 194
Gloucestershire Medical Association resolutions regarding Medical Reform . 563
Guthrie, Mr Charles, his Report on Operations for cure of Squinting 227
Guy, Dr W. A., on the diurnal variations of the pulse in disease 90
Hartle, Dr Robert, on the use of piperine in ague . 98
Harveian Society, notice of the subject of its prize essay . 567
Hair extracted from the anterior chamber of the eye . 553
Henbane, antidote for poisoning by 265
Head, injury of, case of recovery from one . . 261
Hearing. See Ear and Sym . 48
Heart and stomach, case of congenital hernia of . . 251
Hernia, congenital, of heart and stomach . . 251
Holland, Dr G. Calvert, on the properties of the arteries and their influence on circulation . 17
Holroyd, Mr, on the alleged contagion of Plague . 491
Hunter, Dr James, his essay on artificial light . . 247
Infanticide, on its increase in France 269
Invalids, Malta as a residence for 81
Irregular disposition of aorta and pulmonary artery . . 250
Jaundice on that from non-elimination . . 285
Jones, Mr, his observations on the diseases of women . 519
Jurisprudence medical, Dr Traill's outlines of . . 216
Kattywar, on the epidemic fever in 194
Kidney, right, abnormal situation of 549
Kilgour, Dr, his contributions to pathology . . 352
Kutch, fever or plague of . 194
Labour, on the best mode of inducing premature . . 556
Lake-fever of Canada, on 348
Lead, acetate of, cure of poisoning by 558
Lemon-juice, an antidote for poisoning by henbane . 265
Light, artificial, Dr Hunter on 247
Ligature of carotid arteries, on the effects of . . 543
Liver, rupture of, case of 352
Lowe, Dr, on jaundice from non-elimination . . 285
Lucas, Mr, his treatise on strabismus 227
Mackay, Dr Eric, his account of a double monocephalous monster 76
Malta, on, as a residence for invalids 81
Massey, Mr, case of biliary collection in thyroid gland . 454
Materia medica, Mr Pereira's elements of . . 219
Maygrier, his anatomist's manual 244
M'Adam, Mr J. his report on the fever of Kutch . . 194
Menstrual secretion, cause of its not coagulating . . 250
Metacarpus, one of its bones removed without removing the finger 553
Military surgery, on lectures on, in the great medical schools . 565
Milk expectorated at each parturition . . . 268
Mills, Mr C. J. his cases of rupture of carotid artery . 125
Monster, account of a double monocephalous . . 76
——— account of a bicephalous 435
Morehead, Dr Charles. See Bombay Transactions . 194
Mortality of England and Wales, on 157
Muscles, ligaments, and tendons, case of the division of many 264
Musgrave, Mr W. notice of his death 284
Nerves, ganglionic, on functions . 249
——— spinal, functions of roots 546
Neuralgia, action of croton oil in . 100
Newbigging, Dr J. See above . 100
Nosology, statistical, on Mr Farr's 159
Obliteration of lower *vena cava,* 549
Odontography, Mr Owen's treatise on 144
Ophthalmic salt, solution of 260
Opium, on its use after blood-letting 258
Ovum, on the structure of, and changes in . . 474
Paralysis of fifth pair, case of complete 551
Paregoric, Scottish, on . 550
Parturition, delivery of four children at once . 268
Paterson, Dr Robert, literary history of the doctrines of corpus luteum 395
Pathological results in the Mary-lebone infirmary . 304
Pereira, Mr, his elements of materia medica . . 219
Pharmacopœia, the Russian military 137
Phlebitis, on its production - 258
Physical and moral qualities observed in adult age evinced at birth 546

Piperine, on its employment in ague 98
Placenta, on its vascular connection with the womb . . 1
Plague, Dr Bowring and Mr Holroyd on . . 491
Pleurisy, chronic. *See* Empyema 262
Poisoning by antimonial vapours 265
——— by tobacco . 558
——— by acetate of lead . 558
——— by hyoscyamus, lemon-juice an antidote for . 265
———, substances employed in 154
———, cases of . 266
——— by arsenic, on the treatment of . . 559
Procidentia uteri, case of, cured by suture of labia . . 266
Pulse, on the diurnal variations of, in disease . . 90
Quarantine, its efficacy in plague . 491
Rabies, on the poison of, and its transmission . 256
Radford, Dr, his cases of Cæsarean section . . 67
Reform, medical, report on bills regarding . 270, 560, 563
Reid, Dr John, on the blood-vessels of the womb and placenta . 1
——— on the anatomy of the medulla oblongata . 12
——— on the cessation of vital action in asphyxia . 437
Remak, Dr, on the functions of the ganglionic nerves . . 249
Remittent fevers, on those of Canada 348
Report of Registrar-General, . 156
Report of St Mary-le-bone Infirmary 304
Retrospect of practical medicine and surgery . . 542
Retzius ascribes the non-coagulability of menstrual fluid to acid . 250
Revaccinations in France, number of 268
Rhind, Mr William, his cases of division of tendons . 126
Roger and Barth, MM., their treatise on auscultation . 486
Roots of spinal nerves, on the functions of the . . 546
Rupture of liver . 352
——— of spleen . . 355
Salt, common, its solution in ophthalmia . . 260
Sankey, Dr Francis, on Malta as a residence for invalids . 81
Scalp, on the bloody tumour of, in new-born infants . 112
Scarborough, on the vital statistics of 487
Section, Cæsarean, two cases of . 67
———, case of successful 557
Shoulder-joint, case of amputation of, with removal of scapula . 265
Sight impaired, on that resulting from artificial light , 472
Simpson, Dr J. Y., his case of excision of neck of womb . 104
Small-pox, epidemic, account of 191
Small-pox, pustules in internal surfaces . . . 255
Soda, muriate of, solution of, in ophthalmia . . 260
Spinal marrow, section of its right chord, terminating fatally . 553
Spleen, rupture of . , 355
Squinting, Guthrie and Lucas on . 227
Statistics, vital of, Scarborough . 487
Statistical nosology, on Mr Farr's 159
Starvation, deaths by . 180
Stomach and brain, on the sympathetic relation between . 526
Strabismus. See Squinting, and Guthrie, Lucas, and Elliot 227
Stratton, Dr Thomas, on the lake-fever of Canada . . 348
Sym, Dr James, on the mechanical functions of the ear . 3
Tannin, its antisudorific properties 260
Teeth, comparative anatomy of . 144
Tendons, cases illustrating their division . . . 126
Tinctura opii ammoniata, on . 550
Thyroid gland, case of abscess in, containing bile . . 454
Tobacco, on poisoning by . 558
Traill, Dr, his Elements of Medical Jurisprudence . 216
Tumours, bloody, of scalp, on 112
Tympanites, abdominal, on 261
Umbilical chord, on its length and strength . . 556
Urethral inflammation after eating frogs . . 555
Vein, case of rupture of spermatic, causing sudden death . 253
Veins, development of cancer in 251
Vena cava inferior, obliterated . 549
Vertebræ, cervical, dislocation of, 555
Vicarious actions, on their nature 322
Vose, Dr J. R. W., on vicarious morbid actions . 322
Waller, Dr, his treatise on the diseases of females . 519
Warburton, Mr, his proposed bill for medical reform . 270
Whyte, Mr T., on the fever of Kattywar and Ahmedabad . 194
Wightman, Dr, on the sympathy of the brain and digestive organs . 526
Womb, on the connection between and the ovum . 1
—— case of excision of the neck of 104
——— pregnancy and parturition after the same . 469
—— procidentia of, cured by suture of labia . . 266
—— on the danger of injecting fluids into . . 267
Wounds, supra-orbital, not adequate to produce amaurosis . 554
Wylie, Sir J., new edition of the Russian military pharmacopœia 137
Young, Dr W., case of aortic aneurism opening into vena cava . 63

END OF VOLUME FIFTY-FIFTH.

No. CXLVIII. will be published on the 1st of July 1841.

UNIVERSITY OF MICHIGAN
3 9015 06269 1319

IN LIBRARY
27 1912

BOUND IN LIBRARY

FEB 27 1912

UNIVERSITY OF MICHIGAN

3 9015 06269 1319